

SCIENCE.

FRIDAY, FEBRUARY 8, 1884.

COMMENT AND CRITICISM.

THE modern revolution in biology has made it plainer than ever before, that a certain elasticity of scope, a power of adaptation, should belong to scientific foundations. These are usually the outgrowth of enthusiasm, which, at a white heat, is not always so tempered with wisdom as to foresee that the special end then to be met may not forever be of paramount importance. It must be some such explanation as this, which is to be given of the state of affairs recently described by Dr. Harrison Allen of the University of Pennsylvania, as existing in Philadelphia. In the *American* of Jan. 26, Dr. Allen asserts that the existing foundations of Philadelphia are unequal to the present emergencies of biological science, and urges with much force that "an institution for the advancement of biological research, which will be open to both sexes, is imperatively demanded" in that city.

With the widening of the field of biological science, it has come to pass that what we need most at the present time is a new order of things. Academies and museums we must always have; and fortunately, in these respects, we already equal our transatlantic brethren. But that these alone do not cover all of the ground, is evident from the following remarks of Dr. Allen, concerning the Academy of natural sciences in Philadelphia: "The institution is committed to the task of accumulating a reference-library and a museum, of publishing proceedings and occasional memoirs, and affording a reading-room to any and all who are in the remotest degree interested in natural history, and, to this end, to give rudimental instruction at stated intervals to miscellaneous gatherings." All this is well, except that it 'is committed' to this line of

work; and even this would be highly satisfactory, if it were not 'committed' to this alone, as appears to be the case: for the author continues, "The representative members of the academy have acknowledged that the higher education is not within the scope of its work, and have uniformly opposed any attempts at so changing the policy of the society as to admit of any responsibility being unreservedly assumed by its scientific men." That this view should be entertained by the members of organizations instituted long ago, and now endowed with a host of venerable traditions, is, of course, natural; but it is perfectly plain that these alone are no longer sufficient.

Dr. Allen would supply the deficiency which he laments by another foundation,—a biological institute, free from restrictions, liberally endowed, and headed by some one of high repute, qualified especially to inspire and to direct research. We see no reason why a plan like that proposed should not be an immediate and pronounced success, especially in Philadelphia, where science has long been at home, and which is so fortunate as to possess in Professor Leidy an enthusiastic leader and investigator eminently qualified to be the head and front of the new enterprise. We should rejoice to see some such enterprise begun in Philadelphia, particularly if it might enable advanced workers to take immediate advantage of that rich field for zoölogical research in our country which is the admiration and envy of European zoölogists. To this end the endowment should be ample,—we believe, considerably larger than the one hundred thousand dollars suggested by Dr. Allen. It should be, at the least, sufficient to enable advanced workers to proceed to points of timely and special interest; as, for instance, to the Great Lakes, or to the shores of the Gulf,—not to establish laboratories, but to pursue certain lines of research which imperatively require the presence

of the investigator in the field. It is certain that such an enterprise would arouse enthusiasm at home, and command respect abroad.

MR. B. J. LOSSING has recently published a paper on the proposed celebration, eight years hence, of the four hundredth anniversary of the discovery of America. We refer to it now, not to discuss this project, but to call attention to an historical question of such interest that it is worth a thorough investigation. Among mistakes which might almost be classed as popular superstitions must be placed the wide-spread notion that the rotundity of the earth was nearly unknown until comparatively recent times. Mr. Lossing goes so far as to say that the scholars in the time of Columbus ridiculed the idea of the earth being globular, and in this he only echoes the popular belief on the subject. Now, the fact is, that the form of the earth has been as well known as it is now from the earliest historic times, and has never been denied by a scientific writer on scientific grounds. Through twenty centuries of discussion among rival systems and theories, this one has stood undisputed as the fundamental fact of astronomy. Nor has it ever been the subject of religious controversy, as the Copernican theory was. Under these circumstances, it is a question of interest, whether a state of things of which the astronomers never heard existed in Spain four centuries ago; whether, in fact, there are books or documents of any kind showing that men who then ranked as scholars believed the earth's surface to be flat. We suggest the subject to historical investigators.

It must, of course, be understood that we are now speaking of professed scholars, in a position to be consulted by the authorities, and not of the ignorant masses. It is quite likely that Queen Isabella's chambermaid may have ridiculed the idea of the earth being round, and that her spiritual confessor may have looked upon astronomical theories generally as the work of men very dangerous to orthodox religion. But if the knowledge of

an epoch is that of the majority, where shall we stop? It might be found, that, at the present day, the majority of the human race believes the earth to be flat. We leave our readers to picture in their minds an encyclopedia of the thirtieth century, in which it will be stated, that although the astronomers of the nineteenth century knew of the motion of the earth, yet their more numerous and influential contemporaries, the theologians, as represented by one of their leaders named Brother Jasper, believed it to be at rest.

THE acquittal of General Cesnola of the charge of libel, in the case so long before the courts, is probably satisfactory to the trustees of the Metropolitan museum of art, but is far from satisfactory from a scientific stand-point. So far as the trial related to libel, it made no difference to science which side won; but it does make a difference when it appears, that, by legal twists and turns, the vital spot was not touched. As the result stands before the scientific world to-day, the curator, while acquitted of the charge of libel in his hot reply to a former business agent, is still, directly or indirectly, responsible for the manipulations of ancient sculptures in the museum under his charge. One good result may follow from the Cesnola trial. In future, fragmentary objects in museums will probably either be left as found, or else so joined, that, while holding their relative positions, they will still show that they are fragments. The so-called restorations are too often the conceptions of the officers in charge; and, while Cesnola has followed a plan often sanctioned by supposed requirements of art, it is one which will never be permitted by science.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Tropical cyclones.

IN Mr. Davis's paper on whirlwinds, cyclones, etc., in *Science*, vol. ii. pp. 758-761, I notice the use of the term 'equatorial cyclone,' which should be discontinued, as I have already had occasion to state before.¹ There being no deflection of the winds from the normal to the isobars on the equator, there can be no cyclone there; and it is, I think, generally ad-

¹ *Nature*, vol. xix. p. 517.

mitted by meteorologists, that in the latitudes 0° - 60° the deflection is also too small to admit of cyclones; and really I know of none. And even outside India, and the seas around it, there are scarcely cyclones in latitudes lower than 10° .

Thus, what Mr. Davis calls 'equatorial' should be called 'tropical' cyclones. If anybody wishes to mention 'equatorial cyclones,' let him first prove their existence. So long as this is not done, meteorologists having a mind for exact scientific terms will hold to my opinion.

A. WOËIKOF.

St. Petersburg, Jan. 7, 1884.

I shall be well pleased if so distinguished a meteorologist as Dr. Woeikof finds no other points needing correction in my papers on storms than this one. That I fully agree, as to the facts, with him and with Dr. Taylor, who first, so far as I know, states this matter in connection with its cause,¹ is shown in my seventh paper (this volume, p. 40); but, while my use of the objectionable term was accidental rather than deliberate, there is, perhaps, little to choose between 'equatorial' and 'tropical,' both of which occur in this connection in my papers: for, if the first apply in strictness only to points in latitude 0° , the second is equally limited in its exact meaning to points in latitude $23\frac{1}{2}^{\circ}$; and if 'tropical' has come to mean 'within or between the tropics,' so 'equatorial' may mean 'near the equator.' *Tropenzone* of the Germans is not to be translated 'tropical zone,' but 'torrid zone;' and in English, 'tropical' should not be applied in an exact nomenclature to the equatorial low pressures of the doldrums, as in Buchan's writings, but rather to the high pressures of the horse-latitudes, as Ferrel uses it; and 'tropics,' when properly rendered into German, would be *wendekreisen*, or it might be paraphrased into *die polargrenzen der passate*. Inasmuch, then, as the truly tropical belts of the ocean are best characterized by regions of high pressure, free from cyclonic conditions, except where storms from lower latitudes cross them near their western shores; and as the inter-tropical rains of the doldrums are not called 'tropical,' but 'equatorial,' even when off of the equator, and by Dr. Woeikof himself, — it can hardly be considered a serious error to speak of the cyclones, which begin in the doldrums, as equatorial also.

W. M. DAVIS.

Cambridge, Jan. 30, 1884.

Osteology of the cormorant.

Mr. Jeffries' answer in *Science* (iii. 59), to my letter in a former number of this paper (ii. 822), caused me genuine surprise. His suggestion that the occipital style of the cormorant 'is the ossified tendon of some of the extensor muscles of the neck,' made in a former communication (ii. 739), is here, apparently, announced as his conviction, and Selenka is introduced to sustain the statement. Now, I am informed by Mr. Jeffries, that, "in view of such eminent authority, it would seem that something more than simple denial is required to upset a statement accepted by anatomists for many years;" and a few lines farther on, I am said to acknowledge my mistake, because I ignored the point. Permit me to say, that nothing of the kind has been accepted by anatomists for many years. I met this statement by a simple denial, in order to save space in the columns

of *Science*; but, if Mr. Jeffries must be informed as to what the occipital style of the cormorant is, I would inform him that this bone is not an ossification in any tendon of the extensors of the neck, because it is situated, as we know, in the median plane of the skeleton, at a mid-point on the occipital ridge. The tendons of the extensors in a bird's neck, which are inserted at the occiput, are in pairs, their insertion being bilateral; and their tendons are *never* inserted in the median plane: consequently this style cannot be an ossification of any of them. On the contrary, it is an ossification of the fascia between the extensors of the neck and what may be compared to the ligamentum nuchae.

As Mr. Jeffries seems to be anxious about the position in which I drew this occipital style, I would call his attention to the fact that it is shown as occupying its *proper site*, only tipped up somewhat, as it was on my dried skull. Such license is perfectly permissible in anatomical delineation, and is seen in the illustrations throughout the literature of anatomy. It often shows the parts to better advantage; and, in structures as well known as this style is, no explanation is necessary. Acquainted, as I am, with the *anatomy* of this 'nuchal style' and its anatomical relations, I must again acknowledge that I am still ignorant of the *physiology*, or really the function, of this style, or why it should occur in a cormorant and not in other birds nearly related.

As to Mr. Jeffries' concern at my not being, to his mind, thoroughly informed upon the homologies of the patella in birds, I would invite his attention to a paper of mine written some time before my 'Osteology of the cormorant' appeared. To show that I have always agreed with the eminent authorities he alludes to for my benefit, in the co-existence of a patella and an elongated cnemial process of the tibia in most divers, I refer to my article entitled 'The number of bones at present known in the pectoral and pelvic limbs of birds,' in which I say, "I know of but two free bones that occur about the knee-joint. The first of these is the patella; and this may co-exist with the cnemial ridge of tibia, as in *Colymbus* (Owen). The other is a free sesamoid found in some birds in a notch at the head of the fibula (*Speotyto*)" (*Amer. nat.*, November, 1882, 894). I repeat, that 'I find myself misquoted' by Mr. Jeffries, in his remarks upon my paper, 'more than once;' that is to say, he has failed to include statements falsely attributed to my article in the customary quotation-marks. I do not say, (1) that I figure this style '*in situ*,' nor (2) positively affirm that it has never been figured before (ii. 739), but do say, "I do not believe we have a figure showing the site of this bonelet" (ii. 640). Selenka's and Eytton's figures had slipped my mind for the moment, as their works had not been available for a year or more. Furthermore (3), I do not refer to Professor Owen to have him authorize any thing in regard to Podiceps, but only to the patella of the loon, as any one accustomed to anatomical reading can see by referring to my article on the 'Osteology of the cormorant' (ii. 640).

R. W. SHUFELDT.

Upperglow of the skies in relation to halos and coronas.

These striking and beautiful atmospheric phenomena, which have manifested themselves over the entire globe, have attracted much attention, and been minutely described by correspondents in various countries. But there is one feature, which, although incidentally noticed by some writers, has attracted but little attention. I allude to the fact, that, wherever

¹ On tropical hurricanes (Brit. assoc. report, 1852, pt. 2, 31). Herschel used this in his *Meteorology*, but failed to do justice to Taylor's explanation of how a deflective force arises from the earth's rotation, and omits mention of the effect of the conservation of areas, which Taylor recognizes as of essential importance.

the phenomena have been sufficiently pronounced, the sun is during the day encircled by a more or less distinct *colored halo* or *corona*. At this place the assumed supra-cirrus volcanic dust seems not to have been sufficiently dense to have developed the colored rings; and there was observed nothing more than a *whitish glare* extending over the sky from 20° to 25° from the centre of the sun. But the Rev. S. E. Bishop writes me from Honolulu, that this chromatic circle around the sun has been constantly observed in all of the Hawaiian Islands for several months. It has likewise been observed in England as a frequent accompaniment of a conspicuous manifestation of the upperglows of sunset and sunrise.

It is an interesting question, whether this more or less distinct colored zone encircling the sun is a true *ice-crystal halo*, or a *diffraction corona*. Its want of sharp definition, and the absence of the regular succession of prismatic tints due to refractive dispersion, would seem to point to diffraction as the true cause of the chromatic phenomena. On the other hand, the large size of the colored circle, having a radius of from 20° to 30° , would seem to connect it with the well-known ice-crystal halo of about 22° radius.

While I am disposed to regard this chromatic feature of the phenomena as mainly due to the diffractive action on light of the impalpable dust-particles suspended in the lofty supra-cirri regions of the atmosphere, yet it is by no means improbable that ice may be associated with the phenomena: for it appears from the experiments of M. Coulier, and more particularly from those of Mr. John Aitken, communicated to the Royal Society of Edinburgh, Dec. 20, 1880 (*Nature*, vol. xxiii, pp. 195-197; also vol. xxiii, p. 384), that the presence of dust-particles in the air is essential to the formation of fogs and clouds; that, when aqueous vapor condenses in the atmosphere, it always does so on some solid nucleus; and that the dust-particles in the air form these nuclei. Now, it is evident that the presence of these attenuated dust-particles in the supra-cirri regions of the atmosphere would produce condensation of the rarefied aqueous vapor at these lofty altitudes. But inasmuch as this region must, even within the tropics, be far above the plane of perpetual congelation, the condensed vapor must necessarily assume the form of aggregations of ice around these nuclei: hence the diffractive coronas may be associated with imperfectly developed ice-crystal halos. JOHN LECONTE.

Berkeley, Cal., Jan. 25, 1884.

Inheritance of physical injuries.

Well-authenticated instances of the inheritance of a physical injury are so rare, that I wish to put upon record one which has recently fallen under my observation. A gentleman, when a boy about seven years of age, had the second toe of the right foot deformed by wearing a tight boot. The first and third toes were crowded together, forcing the second one under and backwards, and causing a curvature of the second joint, which, in time, became permanent. The joint, being somewhat elevated above those of the other toes, received the pressure of the shoe, and always after was more or less troublesome in consequence. The gentleman was twice married. By his first wife he had six children, the second of which was a daughter; the rest, sons. The daughter inherited the crooked toe; but the feet of all the sons were normal. The deformity appeared, however, in the son of one of these, — the brother next younger than the sister, — affecting the same foot and toe as on the grandfather. By his second wife the gentleman had

only one child, a son, who also inherited the peculiarity; but in this instance it was the second toe of the *left* foot, instead of the right, that was affected.

Knowing that much doubt still exists whether the results of a slight physical injury, like the one I have described, are ever transmitted, I have taken pains to examine carefully all the evidence under my observation; and I feel assured of its correctness. All four having the deformed toes are now living, and all agree upon the facts. The gentleman is positive that his feet were normal until he was about seven years old, and says he remembers very distinctly wearing the boots which caused the deformity. An examination of the foot does not show any congenital peculiarity which might have been transmitted. The toe, when restored to its correct position, appeared normal in every way. No peculiarity of this kind has ever appeared in any other of the gentleman's relatives. I can see no way, then, of avoiding the conclusion that the injury, or rather its results, have been transmitted to two generations.

The case presents some features which render it especially interesting. The peculiarity's appearance in the children of both wives seems to eliminate altogether the element of the mother's influence in producing it. The recurrence of the variation in the grandchild, the father being normal, indicates how powerful was the tendency to perpetuate this slight deviation from nature's standard. In the other cases which I have studied personally, if a variation did not appear in a child, that child's children were free from it also. I should be glad to know if any one of your readers has observed this tendency toward reverting to the ancestral type under similar circumstances. IRVING P. BISHOP.

Perry, N.Y., Jan. 28, 1884.

Pumice from Krakatoa.

Capt. A. W. Newell, of the bark *Amy Turner* of Boston, has brought in some pumice which was washed aboard his vessel, Sept. 17, 1883, in latitude $7^\circ 25'$ south, longitude $103^\circ 21'$ east, about a hundred and sixty-five miles south-west from Krakatoa, Sunda Straits. It covered the sea in windrows, and was observed as fine ashes as far distant as thirteen hundred and fifty miles from its source.

A piece about seven inches by five, which came to my notice, is of a reddish-gray color, and very much inflated: it carries porphyritic crystals of plagioclase feldspar, in many cases surrounded by dark-brown glass, forming small black spots in the gray mass, which might at first sight be mistaken for augite or hypersthene. There is, besides, dark-green augite and brown hypersthene, which is strongly pleochroic, and resembles closely that found in the lavas from the volcanoes of northern California and the Cascade Range (Notes on the volcanoes of northern California, Oregon, and Washington Territory, *Amer. journ. sc.*, September, 1883).

The percentage of silica for this pumice was found to be 62.53, and is almost identical with that of the hypersthene-bearing pumice from Mount Shasta, which is 62. It is undoubtedly the pumice of a hypersthene andesite, and is especially interesting because of its similarity to rocks found on the western coast of North America. The observations of Rénard on the ashes that fell in Batavia soon after the eruption of Krakatoa (*Nature*, Dec. 6, 1883) show the same component minerals, and have doubtless been made on similar material.

JOS. P. IDDINGS.

U. S. geological survey, New York,
Jan. 30, 1884.

THE EVOLUTION OF THE CEPHALO-
PODA.¹—II.

THE individual coiled shell of every existing *Nautilus* may be said to pass through the stages of the protoconch, when it is always nearly or quite straight; then through the first of the conch, when it becomes slightly curved; then through a more completely curved period, in which the first whorl of the spiral is completed. After this it continues the spiral, the whorls on the outside touching the exterior of the inner ones, and spreading so rapidly by growth as to begin to envelop them, and, in extreme cases, to completely cover them up.

The natural inference from these facts would be, that there was a similar succession of forms in past times,—the straight in the most remote, the arcuate and the gyroceran in succeeding periods, and the nautilian only in comparatively modern times. This would be a perfectly clear and legitimate mental conception. The structural relations of the adult shells appeared also to demand the same solution, as shown by the researches of Quenstedt, Bronn, and Barrande, and later of Gaudry. Barrande's researches also demonstrated that this idea could not be maintained, and that there were no such serial relations in time, but that the whole series of forms were present in the earliest period, and occurred side by side in each paleozoic formation. This great author's conclusions have had a curious effect upon paleontologists. It has been hastily assumed by some, that the mental conception was more perfect than could be realized in nature; by others, that the imperfection of the recorded succession was an obvious refutation of the doctrine of evolution, and all pursuit of a solution unworthy of serious attention.

Statistically, the logical picture coincides with the observed succession in time. The straight cones predominate in the Silurian and earlier periods; while the loosely coiled are much less numerous, and the close coiled and involute, though present, are extremely rare. The loosely coiled and close coiled gain in numbers in the carboniferous, and the involute are more numerous than in the Silurian; while, in the later times of the Jura, all disappear except the close coiled and the involute, there being a decided predominance of involute shells. Thus we are able, by reversing the record and travelling back to the Silurian, again to see, that antecedent to that period, in the protozoic, there must have been a time when the straight cones or their immediate an-

cestors predominated, to the exclusion of the coiled and perhaps even of the arcuate types or varieties.

The involute shells of the earliest geological times were therefore probably evolved from the straight cones in regular succession; and we may perhaps hope to eventually get the evidence of this succession in the formations. The exact counterpart of our logical picture, as Barrande¹ has truly stated, does not, however, exist in the known geological records of later periods. Judged by the common classification, by the prevalent ideas about the affinities of adult structures, and by the modes of occurrence of fossils in the geological formations, the forms seem to be without law or order in their succession.

But let us imagine, during the paleozoic, a different condition of affairs from what is now the general rule. Let us suppose such a thing possible as the quick evolution of forms and structure, and that in these ancient periods, near their points of origin, animals found the earth comparatively unoccupied, and were not only able, but in fact forced, to migrate in every direction into different habitats, and to make perpetual efforts to readjust their inherited structures to the new requirements demanded by these comparatively unoccupied fields. Food and opportunity would have acted, in such localities, as stimulants to new efforts for the attainment of more perfect adaptation and for changes of structure useful to that end. We can neither imagine the effort to change of habitat and habits, without its cause, the primary physical stimulant, nor the change of structure, except as a result of the direct effort to meet the physical requirements with corresponding or suitable structures.

Let us also compare the changes taking place during the whole of paleozoic time with those known to have occurred in certain isolated cases in more recent times; such, for example, as that of Steinheim, where a single species, finding itself in an unoccupied field, proceeded with unexampled rapidity to fill its requirements by the evolution of new series and many species, all differing from each other, but all referable, by intermediate varieties, to the original form,—in this example, really a single species, the well-known *Planorbis aequi-umbilicatus*.

¹ We regret that space does not permit some account of the author's wonderful book, the *Système silurien de la Bohême*. While opposed to almost every theoretical conclusion and the general arrangement of the facts made by him, we have the strongest feelings of respect and admiration for his powers of observation, the technique of his work and publications, and the surpassing unselfishness of his life, spent in the pursuit of what he deems to be vital truths (see *Science*, Nos. 43, 44).

¹ Concluded from No. 52.

If we admit such possibilities, and then find similar phenomena in the paleozoic epoch, we shall no longer need our first picture, but can construct a far more natural one.

The Nautiloidea will not then present themselves as a simple chain of being, but as they really were, — several distinct stocks, or grand series, and each of these grand series divisible into many smaller lines of genetically connected forms. In the Cambrian, or perhaps earlier, some of these do not have close-coiled forms at all; some of them have: but all, except the most primitive series, which are composed wholly of straight or arcuate forms, have some close-coiled species. These we can often trace directly with the greatest exactness, both by their development and by the gradations of the adult forms, to corresponding species among the straight Orthoceratites.

The series we have described above, from Orthoceras to Goniatites, compares closely with any single genetic series of the Nautiloidea, and shows that this ordinal type arose very suddenly in the protozoic, and evolved true nautilian shells in the Cambrian or earlier.

The Ammonoidea evolved from the nautilian forms of the Cambrian into series, which are structurally much more distinct from each other in the paleozoic than any groups of the same value (i.e., genera) in the succeeding formations, and thus, in different but equally plain characters, teach us that they also had a quicker evolution within that period itself than in the later formations. Either this was the case, or else the Ammonoidea must have been created in full possession of an organization only attained by similar parallel series of congeneric, close-coiled nautiloids, after passing through all the intermediate transformations above described. Here is a curious fact: though taxonomically equal, we cannot compare the order of the Ammonoidea with the whole of the Nautiloidea, but only with a more or less perfect single series of that order. This phenomenon fully accords with the true picture of the genetic relations. The remarkably sudden appearance and fully developed structures of these earlier ammonoids finely illustrates the fan-like character of the evolution of forms from chronological centres of distribution, and the quickness with which they must have spread and filled up the unoccupied habitats.

After the paleozoic, no absolutely new structural modifications are produced; though the complication of the structures is carried so much farther that we are at first apt to imagine that there are several new types of structure in the trias and Jura. We can carry out

this assertion, even into some minute structural characters. Thus the mesozoic ammonoids have, in all forms, a curious little short collar, which arises from the septa, and surrounds the siphon. It seems to be useful simply to close the joint, and perhaps make the connections of this tube more perfect, and exists in no nautiloid at present known. It was supposed from its development, etc., to be confined to the Ammonoidea of periods later than the paleozoic, but has recently been noted by Beyrich in a Goniatites of the carboniferous. We have found in a similar way every distinctive structural peculiarity of the mesozoic Ammonites appearing in some form among the Goniatites of the carboniferous.

The contemplation of the wonderful phenomena presented by these series has finally led the author, not without reluctance, to the conclusion that the phenomena of evolution in the paleozoic were distinct from those of later periods, having taken place with a rapidity paralleled only in later times in unoccupied fields, like Steinheim.¹

The hypothesis of Wagner, that an unoccupied field is essential for the evolution of new forms, gains immensely in importance, if, as we suppose, it is practical to apply it to the explanation of the phenomena we have observed. Every naturalist must see at once, by his own special studies, that this is the only reasonable explanation of the frequent rapid development of types in new formations, as well as the sudden appearance of so many of the different types of invertebrates in the paleozoic. Newberry's theory of cycles of sedimentation shows that the sudden appearance of types is inexplicable, except upon the supposition that they retired with the sea between each period of deposit, and again returned after long intervals of absence, or perhaps made their appearance for the first time in a given fauna.

With this explanation and that of Wagner the facts we have observed fully coincide, and, we think, amply explain the phenomena, both of sudden appearance in the first deposits of formations, and subsequent quick development in the necessarily unoccupied habitats. The researches of Barrande, Alexander Agassiz, Bigsby, Gaudry, and many others, show us that this must have been especially true of the paleozoic or of the protozoic, if this supposed period is admitted, as compared with subsequent periods.

We find, then, that, in order to make our

¹ Another statement of these facts in the form of a law of evolution is given in the author's 'Genera of fossil cephalopods' (*Proc. Bost. soc. nat. hist.*, xxii. 1884).

logical and generalized picture of exact correspondence between all the changes in the life of a nautilian close-coiled shell and the life of its own group accord with the facts, we must be careful to limit it to groups quickly evolved, and these exclusively paleozoic.

In 1843 Auguste Quenstedt began researches which ought long ago to have led to this solution. He demonstrated by repeated examples, that among diseased types the most extensive changes of form and structure might take place in a single species, and within the narrowest limits of time and surface-distribution. Quenstedt was thus the first to show that in diseased forms the shell had the inherent habit of reversing the process of growth and evolution, and of becoming more and more uncoiled by successive retrograde steps. Von Buch and Quenstedt, master and disciple, and the author independently of either of these predecessors, in three successive researches, have arrived at the identical conclusion, that these uncoiled shells are truly distorted, or, as we may more accurately express it, pathological forms. They are not, however, rare or exceptional, as one might at first suppose, but occur in numbers and in every grade, — from those that differ but little from the normal forms, to those that differ greatly; from those that are exceedingly confined in distribution, to those which lived through greater lengths of time. But in all cases they exhibit degradation, and are expiring types. The author has repeatedly traced series of them, and studied their young, partly in Quenstedt's own collection. In all cases they show us that great changes of form and structure may take place suddenly; and this lesson could have been learned from Quenstedt's work and example as well forty years since as now: and in all species the young are close-coiled, even in some which are arcuate in the later larval, adolescent, and adult stages. Baculites, the extreme form, is straight, and the young still unknown.

When we attempt to resolve these pathological uncoiled series and forms, which show by their close-coiled young that they were descended from close-coiled shells, we find ourselves without comparisons or standards in the early life of the individual. The laws of geratology — that the old age of the individual shows degradation in the same direction as, and with similar changes to, those which take place in successive species or groups of any affiliated pathological series of uncoiled and degraded forms — here come into use, and serve to explain the phenomena. This correspond-

ence is shown in the uncoiling of the whorls, loss of size, the succession in which the ornaments and parts are resorbed or lost, the approximations of the septa, and position of the siphon. It is quite true, as first stated by Quenstedt and also by D'Orbigny, that every shell, when outgrown, shows its approaching death in the close approximation of the last sutures, the smoothness of the shell, the decrease in size, etc.; but, in order to realize that these transformations mean the same thing as those which take place in any series of truly pathological forms, we have to return to the types in which unfavorable surroundings have produced distortions or effects akin to what physicians would term pathological. This frequently happens in small series of Nautiloidea; and, if we confine ourselves to these, we can make very accurate comparisons: or, on the other hand, in the case of the Ammonoidea, we may trace the death of an entire order, and show that it takes place in accordance with the laws of geratology. Such series, among the Nautiloidea, are abundant in the earlier formations; but they have not the general significance of the similar forms among the Ammonoidea, and can be neglected in this article. There are no known cases of degraded series of uncoiled forms among the ammonoids of the earlier or paleozoic periods: they may have occurred, but they must have been excessively rare. In the trias and early Jura, pathological uncoiled forms are rare among ammonoids, but in the middle and upper Jura they increase largely; and finally, in the upper cretaceous they outnumber the normal involute shells, and the whole order ceases to exist. Neumayer has shown, that a similar degradation occurs in all of the normal ammonoids of the cretaceous, and that their sutures are less complicated than those of their immediate ancestors in the Jura. This proves conclusively, that the degradation was general, and affected all forms of Ammonoidea at this time; since the uncoiled forms are not confined to special localities, as in the Jura, but are found in all faunas so far as known. The facts show that some general physical cause acted simultaneously, or nearly so, over the whole of the known area of the world during the cretaceous period, and produced precisely similar effects upon the whole type as had here and there been noticeable only within limited localities and upon single species or small numbers of species during the previous periods. This general cause, whatever it may have been, acted on the type so as to cause the successive generations of the larger part of the shells to become distorted

smaller and more cylindrical in their whorls, smoother, and to lose their complicated foliated sutures. In extreme cases they became again perfectly straight cones, like the orthoceratitic radicals. So much alike are they, that it is quite common for those who are not students of this group to mistake the degraded Baculites for the radical Orthoceras. This decrease in size, increasing smoothness, and uncoiling, is precisely parallel with the similar transformations taking place during old age in the normal involute shells of the Jura, which, when old enough, also depart from the spiral, or tend to straighten out, and always lose their ornaments, decrease in size, and so on.¹

The universal action of the surroundings, as we now know them, is certainly not exclusively favorable to the continuance of life, and may be wholly more or less unfavorable. It certainly perpetually excites the animal to new and more powerful exertions, and, like perpetual friction, wears out its structures by the efforts which it obliges it to make for the support of the structures in doing work. At first this leads to development, the supply being greater than the demand; but sooner or later, and with unvarying certainty, the demand exceeds the powers of supply, and old age sets in, either prematurely, or at the termination of the usual developmental periods. The remarkable and at present unique example of the Ammonoidea places us in a position where we can see the same process taking place in the whole of a large group, with attendant phenomena similar in every respect to those which we have observed in individual shells of the same order.

In numbers of species and genera, and in the complication of the internal structures and the production of the external ornaments on the shells, the order reaches what appears to be the highest stage of development in the Jura; then retrogression begins, and, steadily gaining, finally affects all forms of the type, and it becomes extinct. Smaller series of the Ammonoidea and Nautiloidea go through the same process in their respective time-limits, and in the same way, but can be compared with the individual much more accurately and closely. It is evident, then, that the comparison of the life of an individual with that of its immediate series or group reaches a high degree of exactitude, and that the observed phenomena of the life of an individual should

enable us to explain, in some measure, the equivalent phenomena of the life of the group; and we are unavoidably led to entertain the expectation that it does explain it. This expectation was actually formulated as a probable law for the whole animal kingdom by Haeckel in the same year (1866) as the author first published on the Tetrabranchiata. We are therefore able to quote this leader in science in support of our weaker knowledge; and also a pupil of his, Wurtemburger, who has announced the same results attained by researches on the Ammonites of the Jura, but, naturally perhaps, omitted to recognize any one but his honored master.

The evidence is very strong, that there is a limit to the progressive complications which may take place in any type, beyond which it can only proceed by reversing the process, and retrograding. At the same time, however, the evidence is equally strong, that there are such things as types which remain comparatively simple, or do not progress to the same degree as others of their own group. Among Nautiloidea and Ammonoidea these are the radical or generator types. We have no case yet of a highly complicated, specialized type, with a long line of descendants traceable to it as the radical, except the retrogressive: but all our examples of radicals are taken from lower, simpler forms; and these radical types are longer-lived, more persistent, and less changeable in time, than their descendants.

We find the radicals of the Nautiloidea living throughout the paleozoic, and perpetually evolving new types in all directions; then this process ceases, and the primary radicals themselves die out. But they leave shells, which are in that stage of progression which I have called the nautilian. These, the more direct descendants of the radicals, become secondary radicals, and generate series having more involute shells. These, in turn, as secondary radicals, exhibit very decidedly a greater chronological distribution than their descendant involute forms, persisting, even to the present day, in *Nautilus umbilicatus*. The same story may be told of the Ammonoidea, but substituting at once the close-coiled shell (the secondary radicals) for the primary radicals of the Nautiloidea, even as far back as the Cambrian. These secondary radicals, greatly modified but still carrying in their simpler organizations and mode of coiling the possibilities of a number of new series, existed by the side of the expiring degraded forms of the cretaceous.

This is the essential element of difference between the life of the whole order and that of

¹ We are aware of the existence of evidence that Ammonites of the normal form, the types of which we have seen, have been described from the lower tertiaries: but there are still doubts about the reputed age of the formations; and, in any case, they only tend to confirm the general trend of the facts.

the individual. We can accurately compare the rise and fall of the individual and its whole cycle of transformations with that of any of the single series or branches of the same stock which become highly specialized and then degenerated; but, when we attempt to go farther, we meet with similar difficulties to those encountered in tracing the progress of types and orders. The radical and persistent types are still present, and teach us, that, as long as they exist sufficiently unchanged, new types are a possibility. We have traced many of these in the two orders, and have found that they change and become more complicated, and that probably a purely persistent or entirely unprogressive type does not exist among the fossil Cephalopoda. The most celebrated example of unchanging persistency has been, and is now supposed to be, the modern *Nautilus*. We think, however, that when our observations are fully published, it will become evident that the similarities of this shell to some of the Cambrian coiled forms — which have caused Barrande and others to suppose that it might be transferred to the Cambrian fauna without creating confusion — belong to the category known to the naturalist as representation; that is, similarities of form, and even of structure, in the adults, but with young having entirely distinct earlier stages of development, and belonging to distinct genetic series. Still, comparative unprogression or persistency is common in all radicals; and they force us to recognize the fact, that the orders could have produced new series, perhaps even in the cretaceous, if it had not been for the direct unfavorable action of the physical changes which then took place, so far as we now know, over the whole earth.

Thus, in making our comparisons between the life of the individual and the life of the group, we cannot say that the causes which produced old age and those which in time produced retrogressive types were identical: we can only say, that they produced similar effects in changing the structures of the individual and of the progressive types, and were therefore unfavorable to the farther development and complication of these types. In their effects they were certainly similar; but in themselves they might have been, and probably were, quite different, agreeing only in belonging to that class of causes which we distinguish as pathological, or those whose nature can be generally summed up as essentially unfavorable to the progress, and even to the existence, of the organization.

In order to understand the meaning of these

evidently degraded structures, we must turn back to our first remarks upon the order. The apertures and forms of the retrogressive shells all show that they were exceptional, that they had neither well-developed arms for crawling nor powerful pipes for swimming; that, in other words, they could not have carried their spires in any of the ordinary ways. Their habitats, therefore, must have been more or less sedentary; and like the sedentary *Gastropoda*, as compared with the locomotive forms, they presented degeneration of the form and structure of their higher and more complicated ancestors. Their habitats did not require the progressive grades of structure, and they dispensed with or lost them; and in many cases this took place very rapidly. This retrogression was in itself unfavorable to a prolonged existence; and the geratologous nature of the changes tells the same story, so that we can attribute their extinction to the unfavorable nature of their new habitats, and also call them pathological types without fear of misrepresenting their true relations to other forms.

We have necessarily avoided even allusions to some of the most important confirmatory facts; but we hope our effort will at least show that the theory advanced is a reasonable one, and that the fossil Cephalopoda are worthy of the attention of even the most enthusiastic of the young disciples of the modern school of embryology. The theories of this school will have to stand tests of which they have now not even a faint idea, and it is to be hoped they will not long neglect the precaution of knowing also the past history of the types they often so incautiously and confidently handle.

ALPHEUS HYATT.

THE MOTION OF WAVES OF COLD IN THE UNITED STATES.

THE chief signal-officer of the army desiring to learn the progress of waves of cold across the United States, an investigation has been undertaken in order to determine the appearance of such waves, their approximate velocity, and general line of advance. It would seem, at first sight, as though the problem might be solved by drawing isotherms (i.e., lines through points at the same temperature) on consecutive days, from simultaneous observations over the whole country. If, then, there were a progressive motion, the study of these lines would show it. It has been found, however, that a cold wave does not travel in a well-defined closed curve; and, more than that, the gradual increase of temperature, as the curves approach

the south, masks and often obliterates the motion we seek to find. Again: such waves are frequently divided, then united, thus by the loss of their identity making it impossible to trace them for a long period. One of the simplest methods of procedure in an investigation of this nature would be the projection of the observations of temperature in curves, one for each station, and then studying the fluctuations from station to station. This was done by Professor Elias Loomis, in his ninth paper, in which he investigated the motion of waves of high and low pressure.

An investigation of this kind, a short time since, gave 19.8 days for the mean interval of time of sixteen waves, moving from St. Michael's, Alaska, to Turuchansk, Siberia, along the sixty-fifth parallel, or an approximate mean velocity of 15.8 miles per hour. Such a determination, however, cannot be regarded as entirely satisfactory, because it simply takes into account a series of stations lying in an east and west direction.

In order to extend the investigation to a large number of stations, we may take daily 'departures' from the monthly mean, and, projecting these upon charts, determine the character of the fluctuations over a large area. In practice, however, this method fails, for the reason that the fluctuations diminish toward southerly latitudes, thus masking the progressive motion.

The following method has been adopted for obviating the latter difficulty. We may consider, that if a cold wave advance in any direction, without disturbance from dense clouds or mountain ridges, it will carry minimum temperatures to successive stations in its path; the intervals of time between the passage of such a minimum over any one station taken as a starting-point, and others in the line of progress, gradually increasing. By determining, then, the time of passage of a minimum across each station in a country, and charting these times, we can ascertain both the line of advance and the velocity of the wave.

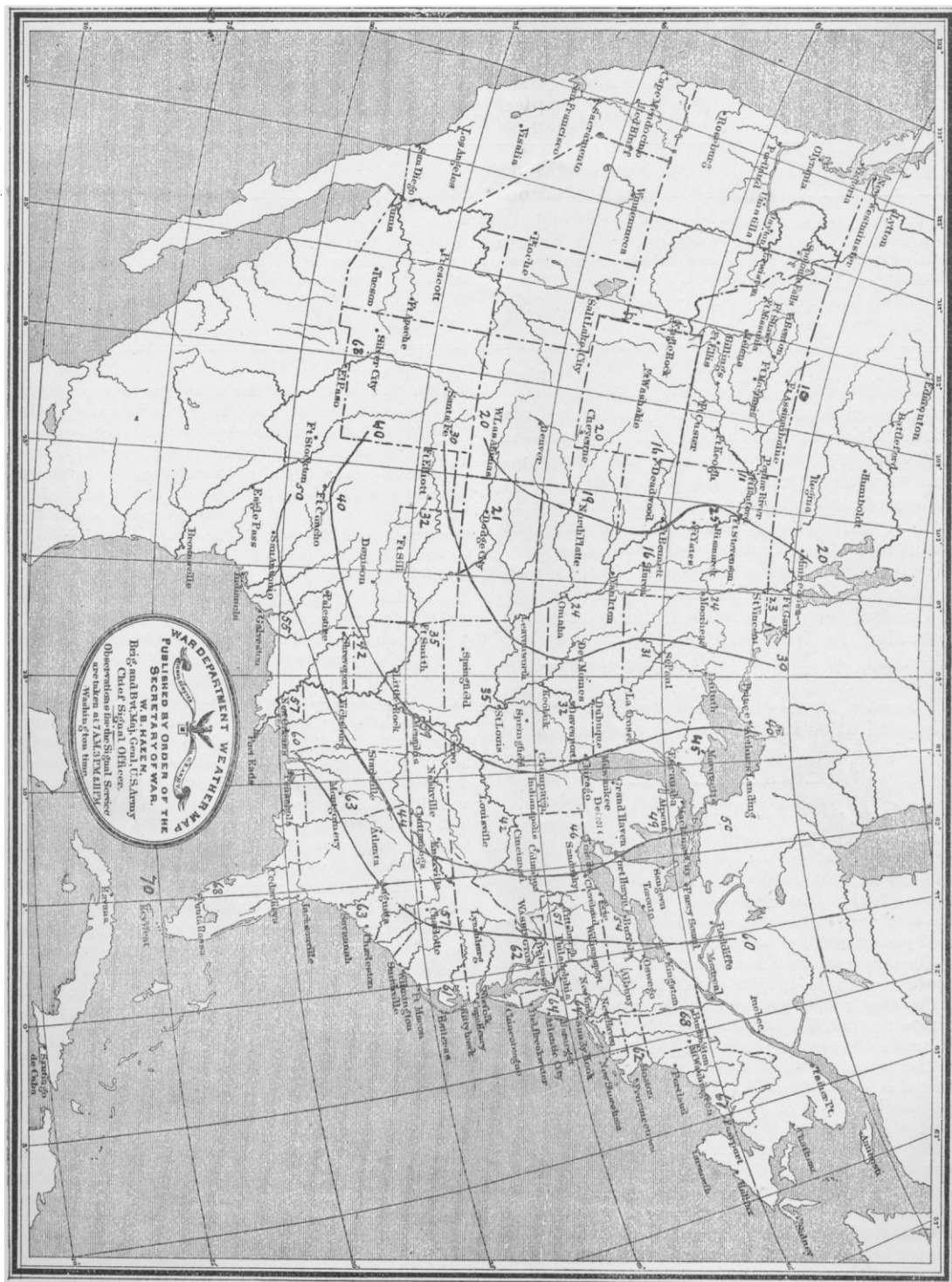
In order to obtain the time of passage of a minimum temperature over a station, where a series of observations has been made each day, it is essential first to eliminate the effect of diurnal range. This may be done by obtaining the residuals for each observation of a month, taken at any hour; then, determining the approximate time of passage, we can, by examining the successive residuals near that time, obtain the time sought. An effort has been made to apply the above principles to the observations of the U. S. signal-service, taken

five times each day during November, 1881, at forty-two selected stations. In this month there were four prominent cold waves; and the following table gives the interval of time which elapsed between the passage of each of these over Fort Dunvegan, North-west territory, and each of the forty-two stations. These figures are inserted exactly as determined from the observations. It was found, however, that many of the apparent discrepancies in a progressive law of motion were due to the appearance of clouds at the time of an observation, thus throwing the minimum forward or back four and even eight hours. Blanks indicate that the minimum could not be determined satisfactorily.

Cold waves, I., II., III., and IV., of November, 1881.

Station.	Hours between Fort Dunvegan and stations in U. S.				
	I.	II.	III.	IV.	Mean.
Alpena, Mich.	48	32	52	64	49
Bismarck, Dak.	24	12	16	48	25
Boston, Mass.	64	52	60	72	62
Brownsville, Tex.	48	64	32	64	52
Buffalo, N. Y.	56	56	48	76	54
Burlington, Vt.	64	-	-	72	68
Cape May, N. J.	48	72	64	72	64
Charlotte, N. C.	56	48	52	72	57
Chattanooga, Tenn.	32	48	40	56	44
Cheyenne, Wyo.	-	-	12	28	20
Cincinnati, O.	32	28	52	56	42
Concho, Tex.	-	-	32	48	40
Davenport, Io.	24	24	32	48	32
Deadwood, Dak.	-	0	8	40	16
Dodge City, Kan.	32	8	12	32	21
Eastport, Me.	72	56	64	76	67
El Paso, Tex.	80	72	56	64	68
Fort Assinaboine, Mont.	24	0	-	24	10
Fort Buford, Mont.	20	0	-16	40	11
Fort Elliott, Tex.	32	16	32	48	32
Fort Gibson, Ind. Ter.	32	24	28	56	35
Galveston, Tex.	72	48	48	52	55
Huron, Dak.	-	8	8	32	16
Key West, Fla.	64	-	-	76	70
Kitty Hawk, N. C.	56	56	56	76	61
Marquette, Mich.	40	48	40	52	45
Memphis, Tenn.	32	24	48	52	39
Montgomery, Ala.	56	-	56	76	63
Moorhead, Minn.	24	24	0	48	24
New Orleans, La.	-	48	52	72	57
North Platte, Neb.	-	8	16	32	19
New York, N. Y.	48	72	56	80	64
Omaha, Neb.	-	8	24	40	24
Pittsburg, Penn.	52	56	48	48	51
Punta Rassa, Fla.	48	-	84	72	68
St. Louis, Mo.	32	24	32	52	35
St. Paul, Minn.	24	24	36	40	31
St. Vincent, Minn.	24	16	24	28	23
Savannah, Ga.	52	72	56	72	63
Shreveport, La.	-	-	32	52	42
Toledo, O.	28	30	52	76	46
Washington, D. C.	56	48	72	72	62

Projecting the mean interval for the four waves upon a chart (see accompanying plate).



MOTION OF WAVES OF COLD, NOVEMBER, 1881.

THE RATE OF MOTION IS GIVEN IN HOURS FROM FT. DUNVEGAN, B.C.

and taking Fort Assinaboine as a starting-point, we obtain the following lines of advance:—

on descent. The boat, in this case, is connected to the balloon by suspension-cords running obliquely;

HOURS FROM FORT ASSINABOINE.

10.	20.	30.	40.	50.
St. Vincent, Minn. Huron, Dak. North Platte, Neb. Cheyenne, Wyo.	Duluth, Minn. St. Paul, Minn. Leavenworth, Kan. Fort Sill, Tex. Santa Fé, New Mex.	Marquette, Mich. Milwaukee, Wis. Chicago, Ill. Memphis, Tenn. Denison, Tex. Concho, Tex.	Erie, Peun. Pittsburg, Penn. Knoxville, Tenn. Vicksburg, Miss. Brackettville, Tex.	Rochester, N. Y. Washington, D. C. Charlotte, N. C. Augusta, Ga. Mobile, Ala.

This shows that in November, 1881, the cold waves were about two days in travelling from Fort Assinaboine to Washington. It would be an interesting comparison if a like investigation were undertaken for waves of heat, also, during other months of the year. A similar method may be applied to the advance of waves of high and low pressure, with the great advantage that clouds would not interfere with the determination of the time of passage.

This subject has attracted much attention from time to time, and recently it has been taken up by Mr. A. N. Pearson of India (*Nature*, Aug. 9, 1883).

The chief signal-officer has kindly permitted this publication in advance of a more extended investigation.

H. A. HAZEN.

TISSANDIER'S ELECTRIC BALLOON.¹—I.

IN describing recently the new hydrogen-gas apparatus which we had constructed in our workrooms at Paris-Auteuil, we mentioned that the governable electric balloon, which has been in preparation since the electrical exposition, was ready for trial. This took place the 8th of last October.

The arrangement of the controllable electric balloon consists of three distinct pieces of apparatus,—the air-balloon, properly so called; the gas apparatus to inflate it; and the electric motor to supply freedom of motion by means of a screw.

The construction of an elongated aerial ship presented serious difficulties. We were aided by two experiments,—that of Mr. Henri Giffard in 1852, and that of Mr. Dupuy de Lôme in 1872. In the model which we tried at the time of the electrical exposition, we arranged for the suspension of the little boat a low rod, running longitudinally similar to that of the air steamship of Mr. Giffard. We afterward concluded that it would be better to place the screw behind a large parallelopiped-shaped boat, high enough to protect the propeller against the danger of a shock

and the deformations of the arrangement are escaped by means of a flexible shaft fixed at either side of the balloon. The balloon was constructed by my brother, in the rooms of Mr. H. Lachambre, to whom was intrusted the making of the new air-ship. A model 15 cubic metres in capacity was first made; and, after studying the action of this in a captive state, the construction of the large balloon (fig. 1) was begun. Its shape was like that of Mr. Giffard's and Mr. De Lôme's balloons: it was 28 metres long, and 9.2 metres in diameter through the middle. On its lower surface, there is a cone with an automatic valve: it is made of a thin cloth, rendered impermeable by a new varnish prepared by Mr. Arnoul of Saint-Ouen-l'Anmône. The capacity of the balloon is 1,060 cubic metres.

The netting over the balloon is formed of ribbons woven with longitudinal spindles, which keep them in their proper geometric positions. The ribbons thus easily adapt themselves to the inflated material, and do not form projections, as do the meshes of a net. The netting is connected on the sides of the balloon with two flexible shafts, which perfectly conform to its shape, passing along the centre of each side. The shafts are made of thin walnut laths fitted with bamboo: they are connected by silk belts. At the lower end of the netting are intersecting rods, at the ends of which are twenty suspension-ropes connected in groups of five to the four upper corners of the car. This latter is in the form of a cage made of bundles of bamboo rods, strengthened by cords and threads of copper covered with gutta-percha. The lower part is made of walnut cross-pieces, which support the willow basket. The suspension-ropes entirely cover the boat: they are woven into the basket, being previously sheathed in caoutchouc, which, in case of accident, protects them from the acid liquid contained in the boat to feed the batteries. The suspension-ropes are connected horizontally by rigging about two metres above the boat. The guide and anchor ropes are attached to this rigging, which also serves to equally distribute the traction during the descent. The rudder, a broad surface of unvarnished silk supported by bamboo, is also arranged behind. The weights of the different parts are as follows:—

¹ Translated from *La Nature*.

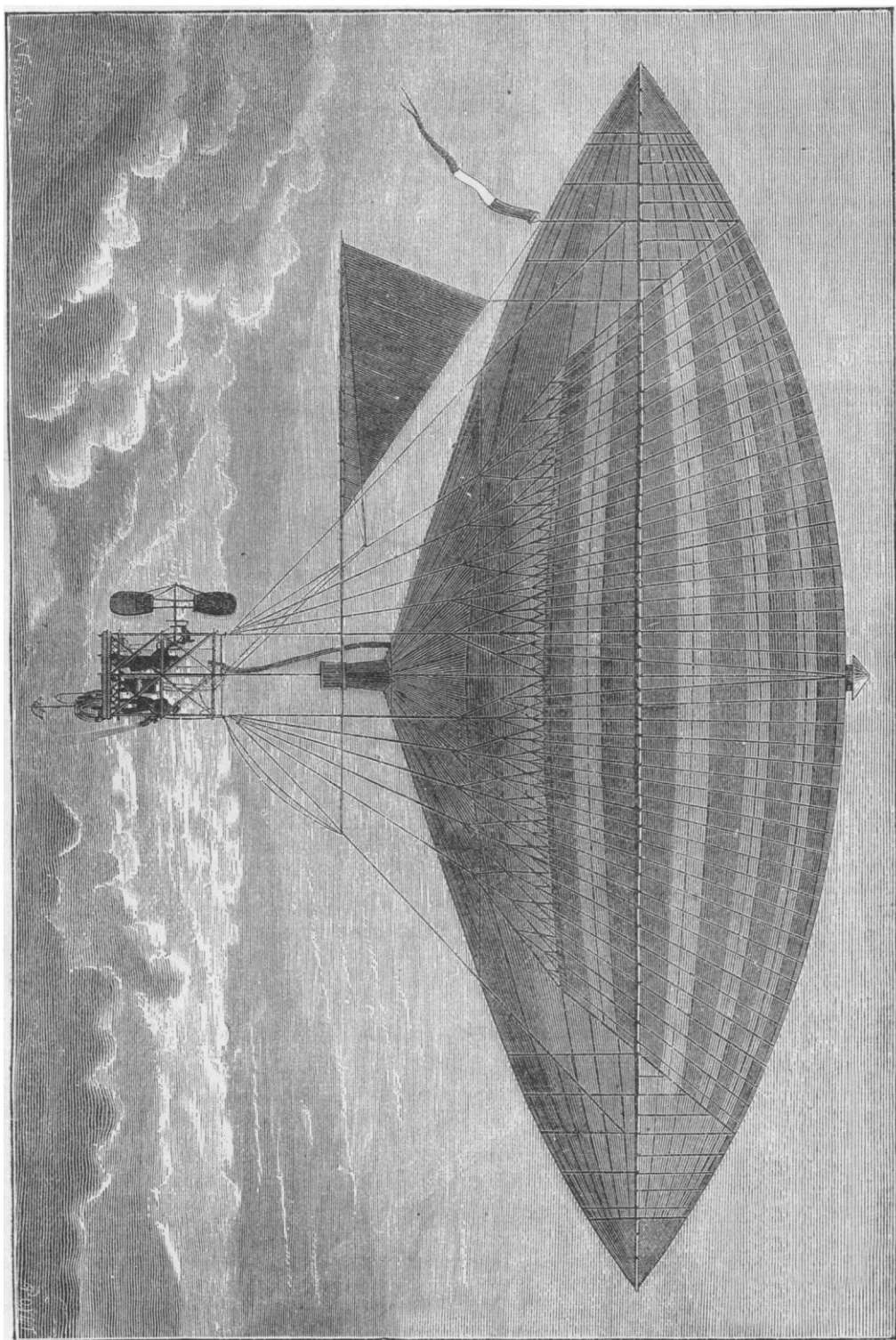


FIG. 1.—TISSANDIER'S FLYING BALLOON AS USED OCT. 8, 1883.

	Kilograms.
Balloon, with the valves	170
Cover, with rudder and suspension-ropes	70
Flexible side-shafts	34
Car	100
Motor, screw, and batteries, with liquid for 2 h. 30 m.	280
Stopping-machinery (anchor and guide rope),	50
Weight of material	704
Two passengers, with instruments	150
Ballast	386
Total weight	1,240

Allowing 10 kilograms, the lifting-force was 1,250 kilograms. The capacity of the balloon being 1,060 metres, the gas had a lifting-force of 1,180 grams per cubic metre, — a result not hitherto obtained in the production of large quantities of hydrogen.

By the end of September the gas apparatus was ready for trial; the balloon was stretched out on the ground under a long tent, in order that it might be immediately inflated; the boat and the motor were stowed under a cart-house; and my brother and I were only awaiting good weather to make the trial. On the 6th of October there was a rise of barometer; on the 7th the weather was fine, with light wind; and we decided to make the experiment the following day, Oct. 8, 1883.

The inflation of the balloon began at eight o'clock in the morning, and continued, without pause, till half-past two in the afternoon. This operation was expedited by means of the equatorial ropes hanging at the right and left of the balloon, and to which were attached the ballast-bags. (The ropes are shown in fig. 2, which also presents the spindle-shaped balloon as seen from one end.) The inflation completed, we proceeded to arrange the boat and the ebonite tanks, each of which contains thirty litres of the acid solution of bichromate of potassium. At twenty minutes past three, having heaped in the ballast and obtained equilibrium, we were slowly raised into the air, a light east-south-east wind blowing. On the ground there was almost no wind; but, as frequently happens, it increased with the altitude; and we found, when the balloon had risen five hundred metres, that it attained a velocity of three metres a second.

My brother was especially engaged in regulating the ballast, in order to keep a constant low altitude. The balloon was kept very regularly at a height of four or five hundred metres. It remained perfectly inflated; and the superfluous gas escaped by opening, under its pressure, the automatic valve, the action of which was very uniform.

Several minutes after the departure, I tried the bichromate of potassium batteries, composed of four troughs with six compartments, making twenty-four elements in circuit. A mercury commutator enabled us to use at pleasure six, twelve, eighteen, or twenty-four elements, and thus to obtain four different speeds of the screw, varying from sixty to a hundred and eighty revolutions per minute. With twelve elements, we found that the speed of the balloon was insufficient; but above the Bois de Boulogne, when our motor was working with great speed, with

twenty-four elements a very different effect was produced. The movement of the balloon became suddenly appreciable, and we felt a fresh breeze produced by our horizontal motion. With the balloon head to the wind, pointing toward the belfry of the church of the Auteuil, near our starting-point, we remained

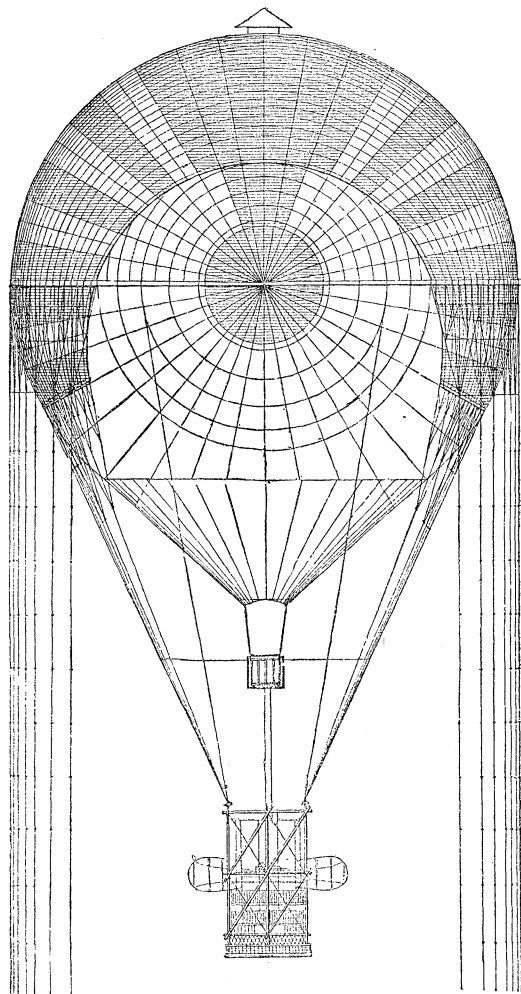


FIG. 2.

motionless, as we proved by noting conspicuous points beneath our car. Unfortunately it did not long maintain this position; but, after acting well for several instants, it suddenly began gyratory motions which the rudder was powerless to completely control. In spite of the rotations which in later trials we were able to prevent, we tried the same experiment for more than twenty minutes, during which we could perceive that we were over the Bois de Boulogne. When we tried to change our position by cutting the wind perpendicularly to its direction, the rudder became inflated like a sail, and the rotations were produced with much greater violence. From this we

assume that the position which an air-ship ought to occupy should be such that its major axis may make with the line of the wind an angle of several degrees.

After the experiments we have just described, we stopped the motor, and the balloon passed over Mont-Valérien. Once, when it had taken the direction of the wind, we began again to turn the screw, proceeding this time with the current. The speed of the balloon was increased, and by means of the rudder we were now easily able to turn to the right or left from the line of the wind. We proved this by taking, as before, some point on the surface; and several spectators also verified it.

At thirty-five minutes past four we made the descent in a large plain near Croissy-sur-Seine. The operation of landing was conducted by my brother with great success. We left the balloon inflated over night, and the next day it had not lost the least gas. Painters and photographers were enabled to obtain views of our air-ship, which was surrounded by a numerous and sympathetic assembly which the novel sight had attracted from all sides.

We had intended to make a new ascent on this day: but, on account of the cold of the night, the bichromate of potassium in our ebonite tanks had crystallized; and the battery, which was by no means exhausted, was on this account, however, incapable of action. We drew the balloon to the shore of the Seine, near the bridge of Croissy; and there, to our great regret, we were obliged to discharge the gas, and to lose in a few instants what had required so much care in its preparation.

Without describing in greater detail our return, we have concluded from this first trial that, 1°, electricity furnishes a balloon with the most convenient power, the management of which in the car is remarkably easy; 2°, in our own case, when our screw, 2.8 metres in diameter, made a hundred and eighty revolutions per minute, we were able to keep head to a wind moving three metres per second, and, when proceeding with the current, to deviate from the line of the wind with great ease; 3°, the mode of suspension of a car from an elongated balloon by means of bands running obliquely, and supported by flexible side-shafts, insures perfect stability to the whole.

We ought to say that our ascent of Oct. 8 should be considered only as a preliminary trial, which will be repeated with the alterations which our experience commends. In addition, we would mention that there was in the car a considerable excess of ballast, and that eventually it will be possible for us to use a much more powerful motor. Aerial navigation will not be made practicable through a single attempt: it will require many trials and efforts and great perseverance under every ordeal.

(To be continued.)

THE DISCOVERY OF THE GERM OF SWINE-PLAGUE.

In a communication read before the Paris academy of sciences, Nov. 26, 1883, by M. Pasteur, the following paragraph occurs:—

"As soon as I received his [Thuillier's] first letters from the commune of Peux, in the department of Vienne, it was certain that he had perceived in the blood and humors of the dead hogs a new microbion which it seemed should be the author of the disease. This microbion had escaped the observation of Dr. Klein of London, in the course of a long and remarkable account of autopsies and experiments published three years before in the report of the English sanitary office. Dr. Klein stated that a microbion was the cause of the affection; but he committed an error, for the microbion that he described has no connection with the cause of *rouget*. Thuillier by his observation had overcome the principal difficulty to a knowledge of this disease of the hog. Historic truth, however, obliges me to declare, that in 1882, and also in the month of March, the microbion of *rouget* was signalled at Chicago, in America, by Professor Detmers, in a paper which does great honor to its author. Thuillier could not have been acquainted with this paper, and I myself only learned of its existence very recently. The observation of the microbion of *rouget* of the hog by Thuillier dates from the 15th of March, 1882."¹

It is so very seldom that investigations on this side of the water receive any notice whatever abroad, and particularly in France, that it seems a pity even to call attention to the very great injustice done to American work in the above statement, since any recognition at all is so much better than being quietly ignored. There is, however, so much of general interest in regard to the gradual development of our knowledge of the germ of this disease, so much of interest in the success and failures of those who have worked upon it, that, aside from our desire to see history correctly written, there is sufficient incentive for tracing the progress of this study, which commenced when the first real light was breaking upon the germ-theory of disease.

Dr. Klein deserves more credit for his share in the discovery of the micrococcus of swine-plague than M. Pasteur seems inclined to grant. In 1876 he published one of the first, if not the very first, reliable microscopic studies of this disease. The care and skill shown in this investigation are more apparent to-day than when the details were first published; and, although he subsequently made the unfortunate mistake of attributing the cause of the disease to a bacillus, this fact should not be allowed to weigh against his former and really valuable researches.²

In his account of the microscopic appearances of the intestine, the following sentence occurs:—

"From and even before the first signs of necrosis of the mucosa, viz., when the epithelium begins to break down and be shed from the surface, there are found masses of micrococci, which in some ulcers occupy a great portion of the *débris*."³

A little farther on he says, —

"There is one more point which I believe deserves careful attention. In the ulceration of the tongue just mentioned, and at a time when the superficial scab has not become removed, I have seen masses of micrococci situate chiefly in the tissue of the papillae, but at some places reaching as far deep as the inflam-

¹ *La vaccination du rouget des porcs à l'aide du virus mortel atténué de cette maladie.* PASTEUR et THUILLIER. *Comptes rendus*, xvii. p. 1164.

² Report on the so-called enteric or typhoid-fever of the pig, by DR. KLEIN. In Reports of the medical officer of the privy council and local government board. New series, No. VIII. Report to the Lords of the council on scientific investigations, etc., 1876, pp. 91-101.

³ Loc. cit., p. 98.

mation extends. That they are micrococci was proved by their forming lumps of uniform granules; these lumps stain deep purple-blue in haematoxylin, and are thus very conspicuous, and besides resist the action of caustic potash, with which all the rest of the tissue disappears. These heaps of micrococci in locality correspond to the papillae, and are on the surface of the scab, but underneath the covering epithelium, some parts of this having changed into a dry, hard, discolored mass, others containing larger or smaller vesicles filled with fluid."¹

In the examination of the respiratory organs we are given even stronger evidence for connecting these organisms with the cause of the disease. In the mucous membrane of the anterior surface of the epiglottis, which was only slightly inflamed in its sub-mucous tissue, he found —

"Lymphatic vessels filled with micrococci. . . . In the infiltrated, firm, more or less disintegrating parts [of the lung] I find great masses of micrococci filling up capillaries and veins, and also contained in lymphatics around arteries.² . . . The pleura is much swollen, and contains great numbers, continuous layers, of lumps of micrococci. The free surface of the membrane is in many parts covered with them. The exudation fluid is also charged with them as has been mentioned above."³

We have here the record of the unbiassed *savant* seeking after the truth, and describing what he sees without any attempt to draw conclusions or build up theories. It was before Koch's brilliant investigations, identifying the *Bacillus anthracis* as the active principle in charbon virus, had seen the light. There was still the greatest doubt as to whether the contagia were essentially animal cells, vegetable organisms, or chemical poisons. It would have been premature to have presented the micrococci at that time as the cause of the disease, though it is evident from these observations that they existed in the tissues of the body before the death of the animal. We have consequently two questions to consider in an inquiry of this kind; viz., (1) Who is entitled to priority for discovering and demonstrating the presence of micrococci in the tissues and liquids of diseased animals? and (2) Who was first in proving the connection between the micrococci and the essential constituent of the virus?

It seems very evident that Dr. Klein discovered the micrococci as early as 1876, but it is equally evident that his investigations were not sufficient to show that this parasite was the cause of the disease. The fact that from later observations, of an entirely different nature, he attributed the cause to another organism, surely can at this day detract nothing from the merits of the paper from which I have just quoted; and it must consequently be acknowledged as a matter of historical truth, the data of which are fully recorded, that Klein discovered the micrococci of swine-plague long before they were seen by Pasteur and Thuillier.

We can now pass to a brief consideration of the investigations which were intended to connect certain organisms found in the tissues or liquids of diseased and dead animals with the cause of the disease.

In 1878 a second and very elaborate report was made by Dr. Klein,⁴ in which he gives experiments

that are supposed to demonstrate the pathogenic nature of a specific bacillus found in certain liquids of diseased hogs, and cultivated for several generations in the aqueous humor from rabbits' eyes. Coming so soon after the publication of Koch's remarkable studies of the life-history of the anthrax bacillus, and agreeing so closely with them in all important respects, it is scarcely to be doubted that the earlier conclusions had more or less influence in shaping the later ones. While it might be interesting to the specialist to enter into details in regard to the defective methods of cultivation used, the unsatisfactory results of the microscopic examination of the tissues and fresh liquids for the bacilli, and the still more unsatisfactory results of the inoculation experiments with the cultivated organisms, our space will not permit this at present. In behalf of a most indefatigable worker, however, I would call attention to the fact that this mistake of Klein's was not so extraordinary as it may appear to many to-day, because the methods of cultivating and studying disease-germs have to a large extent been perfected since that time.

In the same year a number of persons were appointed by the U. S. commissioner of agriculture to investigate the disease known in this country as hog-cholera. The greater part of these served but two months; but Dr. Detmers, having reported the discovery of the disease-germ, was allowed to continue his investigations. In his first report, Dr. Detmers stated that the disease was caused by a bacillus, which he named *Bacillus suis*, because the same, so far as he was able to learn, was peculiar to and characteristic of swine-plague.¹ He saw micrococci, but regarded them as bacillus germs: indeed, he states that he constantly observed one of these under the microscope while it "budded, and grew to double its length, in exactly two hours."²

This report of Dr. Detmers, coming so soon after Klein's, and attributing the virulence to a bacillus of substantially the same characters as that described by Klein, while the latter's micrococci were made to do duty as bacillus germs, — a relation which had been previously ascribed to them by the medical officer in his 'preliminary note,' though it was not suggested by the English investigator himself, — did much to confirm the bacillus theory, and to convince scientific men that the parasite of another contagious fever had actually been isolated, and its connection with the disease demonstrated.

In January, 1880, M. Mégnin published the results of a microscopic examination of the blood in this disease, in which he described and figured a micrococcus.³ This organism existed in single granules, and also in clusters and chains, and agreed so closely with one subsequently studied by me that I reproduced the drawings of it in connection with my report written the following December.⁴

¹ Department of agriculture. Special report, No. 12, 1879, p. 42.

² Loc. cit., p. 53.

³ Recueil de médecine vétérinaire, 1880, pp. 36, 37.

⁴ Department of agriculture. Special report, No. 34, pp. 80, 81, plate IX.

¹ Loc. cit., p. 99.

² *Ibid.*, p. 100.

³ *Ibid.*, p. 101.

⁴ Report on infectious pneumo-enteritis of the pig (so-called pig-typhoid), by Dr. E. Klein, F.R.S. Report of the medical officer of the local government board. London, 1877 and 1878, pp. 169-230.

In his second report,¹ Dr. Detmers does not seem to have materially modified the views referred to above, though he had been studying the disease during the whole of another year. In discussing accepted classifications in his supplemental report, he says, —

"All, however, seem to agree, that those Schizomycetes classed by them under the name of 'Bacillus' do not form clusters or colonies (rasen, zoogloea-masses, gliacoccus, oreocoglia), and do not undergo metamorphoses from globular to rod-shaped Schizomycetes, two things decidedly characteristic of the microscopic parasites of the Schizomycetes family as found in swine-plague; consequently the name adopted, Bacillus, was not well chosen and is not suitable."²

As I have shown elsewhere,³ the two points referred to would not exclude an organism from the genus Bacillus. The best-known bacilli certainly develop from resting spores of an oval form, as seen under the microscope: some of these spores approach very closely to the globular; and, if they should be perfect spheres, the classification would not be affected in the least. The other point — that an organism, multiplying as a micrococcus, after a time develops into a rod-shaped body — is an idea, that, although it is persistently pressed in some quarters, has never been accepted by the best authorities, and is no more true of the organism in question than of other forms of micrococci, as I have assured myself by long series of cultivations. The fact of greatest importance to the present inquiry is, that up to this time Dr. Detmers considered the organism of swine-plague to be rod-shaped in its developed form. This supplemental report, in which the first doubts are expressed in regard to the organism being a real bacillus, was dated six weeks after the appearance of Ménézin's paper, and was not distributed for seven or eight months subsequent to this. It is to be remembered, also, that in none of the above investigations were any sufficient precautions taken to exclude atmospheric germs from the liquids examined, and no pure cultivations were made. It was therefore a matter of considerable doubt whether the organisms described were really in the blood as it circulated in the living animal, or whether they were introduced *post mortem*.

The third report of Dr. Detmers bears the date of Dec. 4, 1880.⁴ In this it was stated that the "swine-plague Schizophytae present themselves in different shape and form." The simplest form is that of a micrococcus. The second form is bispherical: the spherical cell has grown and become contracted, or indented in the middle, forming two united granules.

"These bispherical Schizophytae are always more or less numerous, are either at rest or moving, and usually provided at one end with a flagellum or post-flagellum, which, however, is so exceedingly fine that I have never seen it except with the $\frac{1}{5}$ homogeneous immersion objective of Tolles, and an amplification of over 1,500 diameters, and then only while the Schizophytae was moving."⁵

He then goes on to describe the formation of a chain of bispherical elements, and mentions the existence of zoogloea masses as well. He had not yet given up the rod or bacillus form: for he states that in the blood and pleural exudation, when a day or two old, and sometimes while yet fresh, rod-shaped bacteria can be observed; and it appears probable that the same constitute another form of the swine-plague Schizophytae.¹

The same volume contained a report of mine in which are detailed certain experiments and observations on the schizophytes peculiar to this disease. In this report was given a description of the first successful attempts, as I believe, to demonstrate what micro-organisms, if any, existed in the blood and other liquids of living hogs sick with swine-plague. To keep the liquids to be examined free from all suspicion of contamination, vacuum tubes were prepared by drawing to a point the two ends of a small piece of glass tubing about a fifth of an inch in diameter. A drop or two of water was then aspirated into this tube, boiled to secure a vacuum, and the ends immediately sealed. The tube was now heated to redness to destroy any bacteria spores that might still be in it, and it was ready for filling with the virulent liquid. In use, a very sick hog was killed, a vein laid bare, sometimes before the animal was quite dead, the vacuum tube was passed through the flame of an alcohol lamp, the finely drawn-out end forced into the vein and broken across its walls, when it would immediately fill, and was sealed in the lamp as soon as withdrawn.² It is plain that such tubes could be preserved indefinitely for examination without any suspicion of atmospheric contamination. The only change that could occur would be due to a continued multiplication, — a kind of cultivation of the organisms which had existed in the blood of the living animal.

Three separate outbreaks of swine-plague at widely separated points were investigated; and in every one, I found, by the method of study just referred to, that the virulent liquids contained micrococci, single, and in chains and clusters, but never rod forms, except in those cases where the tubes did not fill well, or where they were imperfectly sealed. And blood from the most perfect of these tubes, which contained no visible organisms but micrococci, produced unmistakable and severe cases of swine-plague in inoculated animals.³ These were the first experiments in which the virulent material, preserved free from suspicion of atmospheric contamination, was shown to contain but a single species of schizophytes; and they were consequently the first which indicated any connection between the micrococci and the essential cause of this disease.

In his fourth report, Dr. Detmers states positively that some of the swine-plague organisms develop a lasting spore, and are changed into a helobacterium.⁴ But there is no account of any measures adopted to decide which of the forms observed in the impure liquids examined had existed in the body of the

¹ Department of agriculture. Special report, No. 22, pp. 13-67.

² Loc. cit., p. 60.

³ Special report, No. 34, p. 68.

⁴ Special report, No. 34, pp. 153-195.

⁵ Loc. cit., p. 187.

¹ Loc. cit., p. 188.

² *Ibid.*, p. 22.

³ *Ibid.*, pp. 23, 24.

⁴ Department of agriculture. Annual report, 1881 and 1882.

living animal; nor was there any substantial reason given for considering the helobacterium as belonging to the same species as the micrococci, or, if they happened to be different, which, if either, was able to cause the disease.

The same volume contains my report bearing the date of Jan. 27, 1882. In this are details of successful inoculation experiments with the sixth pure cultivation of micrococci which had been obtained and cultivated with every precaution known to science at the present day.¹ It was the first real evidence of the pathogenic action of these organisms. It was equally satisfactory with the experiments of MM. Pasteur and Thuillier; and the inoculations were made Jan. 17, 1881, or fourteen months before the discovery of this same organism by these gentlemen.

The communication of Dr. Detmers, referred to by M. Pasteur, appeared in the *American naturalist* for March and April, 1882, and was a *résumé* of his studies for the department of agriculture. In this article he still thinks there is just cause to suppose that the organism of swine-plague has a helobacterium, or rod form, and a resting spore. There are, however, no new observations or experiments referred to, there is no additional proof that the micrococci seen by him were not the result of atmospheric contamination, — nothing to show that a pure cultivation of these would produce the disease. On the other hand, the organism which he describes possesses a flagellum, and a moving stage or period, neither of which have I been able to observe with the true germ of this disease, nor with the closely allied micrococcus which causes fowl-cholera.

It is a matter of record, therefore, that the organism which constitutes the cause of swine-plague was first discovered by Klein in 1876, but that he failed to connect it in any way with the virus of the disease, and afterwards concluded that it depended upon a very different schizophyte. It is also a matter of record that I was the first to demonstrate by satisfactory methods that this micrococcus exists in the blood during the life of the animal, that it can be cultivated in flasks, and that the sixth successive cultivation, made in considerable quantities of liquid, and which contained no other form than micrococcus, still produced the disease. Neither Pasteur and Thuillier, nor any other investigators that I am aware of, have added one particle of evidence, except by way of confirmation, to that previously advanced by me. M. Pasteur is usually very particular in giving credit, but he does not seem to be keeping up with the progress of American science. D. E. SALMON.

MIGRATION OF BIRDS IN ENGLAND.²

THE general report of the committee of the British association, of which this is in fact an abstract, comprises the observations taken at lighthouses and light-vessels, and a few special land-stations, on the east

and west coasts of England and Scotland, the coasts of Ireland, Isle of Man, Channel Islands, Orkney, and Shetland Isles, the Hebrides, Faroes, Iceland, and Heligoland, and one Baltic station (Stevns Fyr, on Stevns Klint, Zealand), for which the committee is indebted to Professor Lütken of Copenhagen. Altogether, a hundred and ninety-six stations have been supplied with schedules and printed instructions for registering observations, and returns have been received from about a hundred and twenty-three, — a result which is very satisfactory, showing, as it does, the general interest taken in the work, and the ready co-operation given by the light-keepers in assisting the committee.

As in preceding years, the line of autumn migration has been a broad stream from east to west, or from points south of east to north of west, and covering the whole of the east coast. In 1880, to judge from the returned schedules, a large proportion of the immigrants came in at the more southern stations; in 1881 they covered the whole of the east coast in tolerably equal proportions; but in 1882 the stations north of the Humber showed a marked preponderance of arrivals. Altogether, a vast migration took place this year upon our east coast; the heaviest waves breaking upon the mouth of the Humber, Flam-borough Head, the Farne Islands, Isle of May at the entrance to the Firth of Forth, and again, after missing a long extent of the Scotch coast, at the Pentland Skerries. The Bell Rock also came in for a share, although apparently a much smaller one than the Isle of May. The easterly winds prevailed all along our east coasts, generally strong to gales; and the succession of south-easterly and easterly gales in October, between the 8th and 23d, occurring as they did at the usual time of the principal migration, brought vast numbers of land-birds to our shores. From the Faroes in the north, to the extreme south of England, this is found to have been the case.

Although migration — that is, direct migration — on our east coast is shown to have extended over a long period, commencing in July, and continuing, with but slight intermissions, throughout the autumn and into the next year to the end of January, yet the main body of migrants appears to have reached the east coast in October, and of these a large proportion during the first fortnight in the month. From the 6th to the 8th inclusive, and again from the 12th to the 15th, there was, night and day, an enormous rush, under circumstances of wind and weather, which, observations have shown, are most unfavorable to a good passage. During these periods, birds arrived in an exhausted condition; and we have reasons for concluding, from the many reported as alighting on fishing-smacks and vessels in the North Sea, that the loss of life must have been very considerable. Large flights, also, are recorded as having appeared round the lanterns of lighthouses and lightvessels during the night migration. From the 6th to the 9th inclusive, strong east winds blew over the North Sea, with fog and drizzling rain; and from the night of the 12th to 17th very similar weather prevailed. Mr. W. Littlewood, of the Galloper lightship, forty miles south-east

¹ Loc. cit., pp. 287-289.

² Report of the committee of the British association for the advancement of science, appointed for the purpose of obtaining observations on the migration of birds at lighthouses and lightships, and of reporting on the same. (From *Nature*.)

of Orfordness, reports, that, on the night of Oct. 6, larks, starlings, tree-sparrows, titmice, common wrens, red-breasts, chaffinches, and plovers were picked up on the deck, and that it is calculated that from five hundred to six hundred struck the rigging and fell overboard: a large proportion of these were larks. Thousands of birds were flying round the lantern from 11.30 P.M. to 4.45 A.M., their white breasts, as they dashed to and fro in the circle of light, having the appearance of a heavy snow-storm. This was repeated on the 8th and 12th; and on the night of the 13th a hundred and sixty were picked up on deck, including larks, starlings, thrushes, and two red-breasts. It was thought that a thousand struck, and went overboard into the sea. It is only on dark, rainy nights, with snow or fog, that such casualties occur: when the nights are light, or any stars visible, the birds give the lanterns a wide berth.

Undoubtedly the principal feature of the autumn migration has been the extraordinary abundance of the gold-crested wren. The flights appear to have covered not only the east coast of England, but to have extended southward to the Channel Islands, and northward to the Faroes (see report, East coast of Scotland). On the east coast of England they are recorded at no less than twenty-one stations from the Farne Islands to the Hanois lighthouse, Guernsey, and on the east coast of Scotland at the chief stations from the Isle of May to Sunburgh Head; at which latter station they have rarely been seen in previous years. Mr. Garrioch, writing from Lerwick, says, "In the evening of Oct. 9 my attention was called to a large flock of birds crossing the harbor from the Island of Bressay; and, on coming to a spot on the shore where a number had taken refuge from the storm, I found the flock to consist of gold-crests and a few fire-crests amongst them. The gold-crests spread over the entire island, and were observed in considerable numbers till the middle of November." The earliest notice on the east coast is Aug. 6; the latest, Nov. 5, or ninety-two days. They arrived somewhat sparingly in August and September, and in enormous numbers in October, more especially on the nights of Oct. 7 and 12, at the latter date with the woodcock. This flight appears to have extended across England to the Irish coast; for on the night of the 12th a dozen struck the lantern of the Tuscar Rock lighthouse, and on the night of the 13th they were continually striking all night. During the autumn, enormous numbers crossed Heligoland, more especially in October. On the night from the 28th to the 29th, Mr. Gätke remarks, "We have had a perfect storm of gold-crests, perching on the ledges of the window-panes of the lighthouse, preening their feathers in the glare of the lamps. On the 29th all the island swarmed with them, filling the gardens and over all the cliff, — hundreds of thousands. By 9 A.M. most of them had passed on again." Not less remarkable was the great three-days' flight of the common jay, past and across Heligoland, on Oct. 6, 7, and 8. Thousands on thousands, without interruption, passed on overhead, north and south of the island too, — multitudes like a continual stream, all going east

to west in a strong south-easterly gale. It would have been interesting if we had been able to correlate this migration of jays with any visible arrival on our English coast, but in none of the returns is any mention made of jays. Subsequently we have received numerous notices of extraordinary numbers seen during the winter in our English woodlands. This seems especially to have been the case south of a line drawn from Flamborough Head to Portland Bill in Dorset. Additions and unusual numbers were also observed at Arden on Loch Lomond side.

The returns show very clearly that the spring lines of migration followed by birds are the same as those in the autumn, but of course in the reverse direction, — from west and north-west to east and south-east. Another point worth noting is the occurrence of many species in spring at the same stations frequented by the species in autumn: thus double records occur at the Mull of Galloway, Bell Rock, Isle of May, as well as at some English spots.

As this is the fourth report issued by the committee, we may, perhaps, with the mass of facts at our disposal, be expected to draw deductions which, if they do not explain, may serve at least to throw some light on the causes influencing the migration of birds. We might reasonably reply that the work undertaken by us was not to theorize, or attempt explanations, but simply to collect facts, and tabulate them. This we have endeavored to do in the shortest and simplest manner consistent with accuracy of detail. There is, however, one circumstance which can scarcely fail to present itself to those who have gone carefully into the reports issued by the committee; namely, the marvellous persistency with which, year by year, birds follow the same lines, or great highways of migration, when approaching or leaving our shores. The constancy of these periodical phenomena is suggestive of some settled law or principle governing the movement. It is clearly evident, from the facts already at our disposal, that there are two distinct migrations going forward at the same time, — one the ordinary flow in the spring, and ebb in the autumn, across the whole of Europe. A great migratory wave moves to and from the nesting-quarters of the birds, in the coldest part of their range, — north-east in the spring, and south-west in the autumn. Quite independent of this, there is a continual stream of immigrants, week by week and month by month, to the eastern shores of these islands, coming directly across Europe from east to west, or more commonly four points south of east to north of west, and the reverse in the spring. These immigrants are mainly composed of those common and well-known species which annually make these islands their winter quarters, and, as a rule, take the place of our summer birds. They come in one broad stream, but denser on some special lines or highways than others. Cutting the line of ordinary migration at nearly right angles, one flank brushes the Orkney and Shetland Isles, pouring through the Pentland Firth, even touching the distant Faroes. The southern wing crosses the Channel Islands, shaping its course in a north-westerly direction to the English coast.

SPANG'S LIGHTNING PROTECTION.

A practical treatise on lightning-protection. By HENRY W. SPANG. New York, Van Nostrand, 1883. 63 p. 8°.

THIS is a new and enlarged edition of the author's treatise on lightning-protection, published in 1877. The book contains altogether too many good things to be bad, and too many bad things to be entirely good. There is a wholesale condemnation of all systems and methods other than those described, which is a little perplexing, until one discovers, and in fact the author confesses, that it is issued with a view of effecting a general introduction of a *patented* system of lightning-conductors. The business air which pervades the whole is thus clearly explained.

The author frequently pronounces against the 'lightning-rod men and scientists,' attributing the blunders of the former to the mistakes of the latter. A brief examination of his book will suffice to fully acquit him of the charge of belonging to the latter class; and it must be confessed, that the many excellent rules which he has emphasized oblige us to rank him considerably above the general average of the former. The general principles of lightning-protection, as presented, are in the main correct; and, as general principles, they deserve a wide dissemination. The particular system urged as the only efficient one is more complicated, and, even if it were not patented, more expensive than is necessary. Some novel statements are made, concerning what lightning 'will do' under certain circumstances, in the same paragraph in which the author bemoans the ignorance shown by scientific men on the subject of lightning-protection.

The author does not seem to be aware of the existence of what is doubtless the most complete and authoritative treatise on the subject yet published, — the elaborate 'Report of the lightning-rod conference,' edited by G. J. Symons, F.R.S. This conference was made up of delegates from the London meteorological society, the Royal institute of British architects, the London society of telegraph engineers and electricians, the London physical society, and two co-opted members, Profs. W. E. Ayrton and D. E. Hughes.

The examination of the various problems presented was exhaustive, and the code of rules for the erection of lightning-conductors published in the report is simple and easily understood. The proper construction of an efficient lightning-conductor is, after all, a matter of no great difficulty, and of comparatively little expense. The inauguration of a proper system

of testing conductors would certainly reveal some astonishing facts in regard to the efficiency of rods as generally erected, elaborate and expensive as they often are. The wide circulation of the rules adopted by this conference would undoubtedly be the means of bringing about a much-needed reform in this direction.

COHN'S 'DIE PFLANZE.'

Die pflanze: Vorträge aus dem gebiete der botanik. Von Dr. FERDINAND COHN, professor an der Universität zu Breslau. Breslau, Kern, 1882. 8 + 512 p. 4°.

THIS elaborately gotten up book of over five hundred pages comes to us as a contribution to general literature, and does not address itself to the scientific botanist, except as he is interested in a popular presentation of botanical facts and problems with which he is supposed to be more or less familiar. Dr. Cohn believes it to be the duty of those versed in any branch of science to produce a literature which shall invite a large circle of readers to an interested acquaintance with their chosen science. "Nor are they to recoil from this task," says he, "because of the difficulties which present themselves for satisfactory solution, or because popular writings on natural science have been undervalued by many." Actuated by his conviction, Dr. Cohn has collected the addresses which he delivered at various places in Germany between the years 1852 and 1881, and, while retaining their original form, has remodelled them sufficiently to bring them up to date and compact them into a shapely whole.

In the preface the author sets forth a difficulty which besets the popular lecturer on scientific topics, — one which doubtless every one who has tried this style of address has fully realized, — namely, the meagre knowledge and hazy comprehension with which the majority of hearers listen to his words, necessitating so long a dwelling on the elementary facts of the topic that little time is left for the consideration of the more recondite and interesting points.

If we may be allowed to judge, Dr. Cohn has overcome this difficulty to a large degree in a very happy manner. He devotes the first lecture, entitled 'Botanical problems,' to a brief history of the development of botany, and an explanation of some of the elementary principles of the science, thus paving the way for subsequent discussion of more special matters. Some idea of the variety of the topics treated may be gained from the titles of the sixteen lectures, which are as follows: 'Botanical problems,' 'Goethe as a botanist,' 'The cell state,'

'Light and life,' 'The plant calendar,' 'From pole to equator,' 'From sea-level to eternal snow,' 'What the forest tells of itself,' 'Grapes and wine,' 'The rose,' 'Insectivorous plants,' 'Botanical studies on the seashore,' 'The world in a water-drop,' 'Bacteria,' 'Invisible enemies in the air,' 'Gardens in ancient and modern times.'

The lectures are written in an entertaining style, and vary in interest as little as the inequality of the subjects will allow. The two on 'Light and life' and 'The cell state' are especially happy, particularly the latter in an apt comparison of a plant to a state.

The design of the book is laudable, and its execution admirable. We commend both as models to our American biologists and physicists, who owe it to the American public to provide better opportunities for a general acquaintance with scientific problems and methods.

REPORT ON SORGHUM-SUGAR.

Investigation of the scientific and economic relations of the sorghum-sugar industry; being a report made in response to a request from the Hon. George B. Loring, U. S. commissioner of agriculture, by a committee of the National academy of sciences, November, 1882. Washington, Government, 1883. 152 p. 8°.

A PROBLEM, which, if not the most important, is certainly the most prominent, agricultural problem of the day, is that of the profitable production of sugar from sorghum. The experiments made during the last few years at the U. S. department of agriculture and elsewhere have attracted general attention, both on account of the interesting scientific questions involved, and still more because they promise to create a new branch of agricultural industry, and to greatly enlarge our domestic supply of sugar.

The report of the committee of the National academy on this subject must prove very valuable to all interested in the promotion of this infant industry, because it contains a very full summary, prepared by thoroughly competent and impartial persons, of all that has been accomplished in this direction up to the date of the report, and thus collects in one publication information previously scattered through numerous state and other reports. That the work has been well done is sufficiently guaranteed by the names of the committee. They were Prof. William H. Brewer, Ph. D., of the Sheffield scientific school; Prof. Charles F. Chandler, Ph.D., of Columbia college; Prof. S. W.

Johnson, M. A., of the Sheffield scientific school; Prof. B. Silliman, M.A., M.D., of Yale college; Prof. J. Lawrence Smith, M.D., late of the University of Louisville; and also, not of the academy, Gideon E. Moore, Ph.D., of New York. Prof. C. A. Goessmann, of the Massachusetts agricultural college, was also a member of, and acted with, the committee until Sept. 15, 1882, when he resigned.

The committee begins its report with several pages of citations from earlier (chiefly American) investigations upon sorghum as a sugar producing plant, showing the conflicting opinions upon almost every essential point of the subject entertained by the authorities quoted. On such points as the kind of sugar present in the juice, the best varieties of sorghum, the proper time for harvesting and working, etc., diametrically opposite opinions, each by reputable authorities, are quoted.

This was the state of the question, when, in 1878, the U. S. department of agriculture, by its chemist, Dr. Peter Collier, began its well-known investigations, which went far to decide many of the points just spoken of. This work the committee does not review in detail, but contents itself with a favorable criticism of the analytical methods employed, and with pointing out the material value of the results and the need of further investigation.

At the time when this report was prepared, the successful work of the department of agriculture consisted chiefly of chemical examinations of sorghum-juice, attempts to produce sugar from it on a manufacturing scale having proved partial failures: the committee therefore closes its report with brief accounts of the results of practical attempts to make sugar from sorghum. Among these are noted two failures, and seventeen cases of more or less pronounced success, several on a manufacturing scale.

In an appendix are collected divers interesting papers bearing upon the subject of the report. Some of them present fuller details of experiments referred to in the report, and some contain accounts of later successes in sugar-production. This portion of the report concludes with a 'Bibliography of sorghum,' which cannot fail to be of great value to investigators in this field.

It is evident from the facts collected in this report, and from the experience since gained, that, with skill in working, sugar can be successfully made from sorghum. It is also equally evident, that, without that skill and the proper appliances, failure is more probable than success. Sirup can easily be made from sorghum on a domestic scale, but not sugar.

Finally this report makes very evident the need for further investigation in regard to such important points as the best varieties of cane, and the possibility of their improvement by selection and crossing, the most suitable soil for sorghum, the effect of fertilizers on its growth and content of sugar, the methods of extracting the sugar from the cane, and the prevention of losses in the further treatment of the juice. In a word, while sugar can be made from sorghum, it yet remains to be seen how economically it can be manufactured, and how completely the great waste involved in the present crude processes can be avoided; and the committee closes its report by urging upon the U. S. department of agriculture especially, the duty of continuing the investigations which have already yielded such important results.

HANN'S CLIMATOLOGY.

Handbuch der klimatologie. Von Dr. JULIUS HANN. Stuttgart, J. Engelhorn, 1883. (Bibliothek geographischer handbücher.) 10 + 764 p. 8°.

THERE are many treatises upon the subject of climate. The larger number of these are devoted to the consideration of the special characteristics of the climate of some particular country, and contain numerous statistics derived from meteorological observations, together with a description of the prevailing weather conditions. A few discuss the subject from a broader stand-point, and take account of the general conditions which prevail over a large area, with their causes and modifications. The treatise before us, however, differs from its predecessors in its aim as well as in its execution. It is designed to give a view of climatology as the result of certain forces which are at work in nature, and to investigate the result of the operations of these forces as they are exhibited in the climate of the world. Its author is the acknowledged head of meteorological science in Austria, — one who has done much to place meteorology on a scientific basis, and who is especially qualified to speak with authority upon the subjects which he treats, on account of his well-known familiarity with the current work of other investigators, and his ability as a critic. It is to be expected that a work written by such an author will be comprehensive, thorough, and masterly, that it will indicate the present condition of the subject from a scientific stand-point, and be as accurate as the best data at hand can make it. All these conditions are fulfilled and abundantly satisfied in the work before us.

The aim of the treatise is to present a comprehensive view of climatology. First the word is defined, its object specified, and the various climatic factors mentioned, briefly discussed, and illustrated. After this introduction, which is, in fact, a concise treatise upon the subject of climatic statistics rather than a simple introduction, the author proceeds to treat the subject in two divisions, — general and special climatology. Under the former head are considered, 1°, 'solar climate,' or that which would result directly from solar radiation; then, 2°, the modifications introduced by atmospheric and terrestrial conditions, resulting in climate as actually existent. Under the latter head are considered the special climatic characteristics of different portions of the globe, with copious illustrations. In carrying out this plan, the author treats the various topics with conciseness but with singular clearness, and advances in logical progression without dwelling too much on the minor details, or retarding the course of thought by discussing the many collateral subjects which are naturally suggested. In a few instances, where a controverted subject is discussed in the text, an elaborate footnote is devoted to a defence of the author's position, or a statement of the dissenting opinions of others; and several appendices contain fuller explanations of the special topics touched upon in the main portion of the treatise. In this way the author preserves the unity of the work, and at the same time calls attention to important considerations to which he cannot give much space in the body of the treatise. The work is not exhaustive: indeed, that would be impossible in so comprehensive a subject. In many cases it does not enter into the details of an investigation, but gives the results obtained without discussing the methods of investigation employed.

At the outset the author carefully defines the word 'climatology,' and shows the relation between climatology and meteorology. By climate is to be understood the average weather conditions of different places on the earth's surface, together with the extent of the deviations from the average conditions. The climatologist, in treating the causes of climate, necessarily makes use of the laws which the meteorologist in his broader study of atmospheric phenomena has deduced, and, in turn, furnishes the latter with facts which he must account for by the meteorological principles he has established. The two sciences are therefore intimately connected; and we may, if we wish, regard climatology as a part of the

science of meteorology, which takes into account the phenomena included in the latter only in so far as they affect the well-being of living creatures on the earth. It is well to establish a definite position for climatology; and the author is wise in restricting it to average weather conditions, and deviations from the average, for these are the controlling influences which determine the relations of any place to animal life. The important climatic factors are temperature, moisture, cloudiness, wind, atmospheric pressure, evaporation and the chemical composition of the air, mentioned nearly in the order of relative importance; temperature, rainfall, and wind being usually given as the three essential factors. These factors are not independent, and are so mutually connected (as, e.g., cloudiness and temperature) that they cannot be discussed separately. Atmospheric electricity is recognized as important, but needing further study before it can be classed as a factor. It would seem as if a similar reason would have prevented the insertion of the composition of the air in the list of factors; for the relation of the chemical constituents in their varying proportions to animal life is confessedly obscure. In the discussion of these factors, those special features with regard to each are mentioned which would be useful in representing statistically the climate of a place. Especial prominence is given to temperature statistics; and eight different subjects are named which deserve representation in tables, such as the monthly and annual means, magnitude of daily ranges, etc. An important omission in the tables usually given is pointed out; viz., some expression for the rapidity of temperature changes. The author suggests two ways in which this can be done, —1°, by the difference between consecutive daily means, and, 2°, by the rapidity of changes in some adopted period of time. These and other suggestions can be profitably considered by those who have charge of the preparation of weather statistics: they should be presented in such a shape as to enable any one to readily obtain the facts as to the climate of any place for which he consults them. The statistics of Vienna are presented in illustration of the subject.

The section devoted to general climatology is of special value. Quite properly, it begins with a discussion of climate as dependent upon the distribution of solar heat, and thus includes the results of investigations in solar radiation. Disregarding for the time the effect of the atmosphere, the distribution of solar heat is treated as dependent upon the sun's altitude,

the length of the day, and the distance between the earth and the sun. The combined effect of these elements is seen in different latitudes and in the two hemispheres, and is the basis of actual climate. The effect of the atmosphere in modifying these results is next considered, and the subjects of atmospheric absorption, diffusion, and reflection treated. For the sake of comparison, the relative effects on light, heat, and chemical power, are considered, though that relating to heat alone properly comes into consideration. The little knowledge which we possess on these subjects is the cause of the somewhat meagre presentation which the author gives. Coming finally to the further modifications in the distribution of solar heat due to the earth itself, the characteristics of insular, continental, and mountain climate are pointed out, and the effect of marine and aerial currents noted. In this way the author arrives at the actual climate which prevails over the earth, having started with its prime source, the sun. The discussion throughout is general, but is very suggestive of further study by the reader, in the different topics treated. The author quotes extensively from the works of others, and gives copious illustrations. It would be a great help to the student, however, if, as has been elsewhere suggested, references to the works themselves were given, in addition to the names of the authors, which are always carefully mentioned. This section could be expanded into a larger treatise, and may serve with advantage as the basis of extended research, or as a help to class instruction.

The section devoted to special climatology occupies two-thirds of the whole work. After an introduction upon the division of the earth into climatic zones, the author considers the observed climate in each zone, quoting extensively from the publications which describe the prevailing conditions in each country, giving numerous tables, and summing up under each zone its general characteristics. In this section we have, therefore, a compendium of accessible statistics covering the whole world, given in as much detail as the generous limits of the work will allow, and combined with carefully prepared summaries. In order to judge of the excellence of this section, it is only necessary to note how thoroughly the different countries are treated, whether the selections made for illustration are typical or not, and whether the author has made use of the most reliable publications. In all these respects it will be found that the work before us excels. Thus, in describing the climate of North America, the author first gives a statement of

the topography of the country, and the important distinctions in climate which result therefrom; then, passing to the climatic factors, he describes in detail the temperature, rainfall, humidity, cloudiness, pressure, and winds as they exist in the different sections, and illustrates principally from the publications of Schott, Woelikoff, Blodgett, Loomis, Coffin, Dall, Gannett, Whitney, and the signal-service. Not content with general characteristics, he further specifies peculiarities, such as the suddenness of temperature changes in certain localities, tornadoes, northerners, and Indian summer, with appropriate quotations from various writers; and he also appends special descriptions of the climate of Illinois, Lake Superior, the Mississippi valley, Canada, Manitoba, Hudson's Bay, Alaska, the plateau region, Colorado, California, Arizona, and the Bermudas. The climate conditions of other countries are treated with similar thoroughness, making the whole valuable for reference, while its chief merit lies in the running descriptions and summaries. A defect in the work is the lack of charts illustrating the various data. A few only are given; the main reliance for illustration being in the statistical tables, which are almost unnecessarily abundant. Graphic representations are always specially valuable to the reader, and their addition to the work would be a real improvement. It would also have been well to mention the analytical method of representing data, as well as the statistical and graphical; for, while its use is limited, it will surely grow in favor with the advance of the science.

The work of Dr. Hann represents the latest investigations, and is brought down almost to the very date of publication. It will therefore not be soon superseded; and, while additional data will accumulate in coming years, the general discussions will require but little alteration. The work is recommended for the general reader, not to be read in course, but by proper

selection. The general chapters and the summaries contain a large amount of information, for which the details and illustrations can be obtained from the accompanying pages. The student will find the work useful in calling attention to the authorities in each subject. Especially in the section on general climatology, where such topics as solar radiation and atmospheric absorption are, from the design of the work, treated in a general way only, will be found quotations from the publications of the latest investigators. It would be well, too, if the treatment of the subject of climatic factors should call attention to the need of publishing statistics in such a way as to be useful for reference. In this country particularly, we need to give consideration to this subject. There is scarcely an allusion made in the work under review to recent meteorological work in the United States, not because it has not been published, but because it has not been issued in a suitable form. In order to compare our statistics with those of other countries, it is necessary first to re-arrange and classify them. The international meteorological committee has recommended forms of publication, the adoption of which will add greatly to the facility with which corresponding data can be compared. But even these forms do not give all the data which the climatologist would like to have; and meteorological observations could be made more available for studies in climate by attention to the author's treatment of the subject. There is also need of deducing more results from the immense collection of data which is daily accumulating all over the world, to check the prevailing tendency of heaping up observations for no useful purpose. If this work shall have the effect of stimulating research, and promoting a more intelligent use of meteorological observations, it will do much good. It is to be hoped that it will be translated into English to reach a wider circle of readers.

RECENT PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Ottawa field-naturalists' club, Canada.

Jan. 31. — Mr. W. L. Scott read the report of the ornithological and oölogical branch, showing that a number of rare birds had been secured, that thirteen species had been added to the published lists, and that other good work had been done. Among specimens exhibited was a great white egret, in full breeding-plumage, which had been shot on the Upper Ottawa, — a locality far north of its usual range, but where it is stated to be a not uncommon visitor. Its

name, however, will not appear in the lists of the club, as the locality is considered beyond the limits of its district.

Prof. J. Macoun read a practical paper on the 'Edible and poisonous fungi' of the vicinity. He pointed out, that while at present the only fungus collected for food is the common mushroom, *Agaricus campestris*, there are other equally nutritious and palatable forms which exist in far greater abundance; as, for instance, *Coprinus comatus*, which grows in great profusion about the city during Sep-

tember. Two species of morel, *Morchella esculenta* and *Gyromitra esculenta*, are also common, and are very desirable food-supplies; while the Lycoperdons, or puff-balls, are found in immense numbers, and often of large dimensions, and, when young, are excellent for the table. Among poisonous forms, the fly-agaric, *Amanita muscaria*, was instanced as one of the most highly organized, most widely distributed, most beautiful, and most dangerous of the agarics. The difficulty of distinguishing by sight between many edible and non-edible or poisonous species was stated; and it was explained that those having a pleasant odor and taste were always likely to be eatable and harmless, while others would be more or less injurious. It is, however, always advisable, at first, to eat but a small quantity of any untried species. Professor Macoun, as botanist to the Geological survey, is now working at the fungi of Canada, and is preparing a report thereon for publication. Beautiful plates to illustrate this report have been drawn by Mrs. Chamberlin, a member of the club; and such as related to the forms discussed were exhibited by her, to the great gratification of the members. — The secretary laid on the table advance copies of the Transactions of the club for 1882-83, and announced that they would be ready for distribution in a few days.

Princeton science club.

Jan. 25. — Professor Rockwood gave a paper on the mutual relations of the conics, discussing especially the changes by which the different curves turn one into another through their limiting forms. He traced the various curves represented by the equation $A^2y^2 + B^2x^2 = A^2B^2$, when A^2 is constant, and B^2 assumes all possible values, showing them to include all the varieties of the ellipse and hyperbola. In the same way the equation $y^2 = \frac{2B^2}{A}x - \frac{B^2}{A^2}x^2$ was shown to include all forms of the conic when A and B assume various values, — zero, infinite, or finite. In tracing the movement of the foci, he showed that in the series of ellipses and hyperbolas the foci come first to coincide at the centre; afterward their distance from the centre becomes $c\sqrt{-1}$, which, by interpreting the imaginary factor as indicating revolution through a right angle, changes the foci from the horizontal to the vertical axis, on which they then recede to infinity, returning from infinity on the horizontal axis when the ellipses change through two parallels to the series of hyperbolas. The changes were also followed out with focus and directrix fixed and eccentricity varying, with focus and focal ordinate fixed and eccentricity varying, etc.

Professor Scott reported that he had just discovered a rudimentary pollex in *Oreodon* of the White River miocene. The carpus is very primitive in arrangement, and a trapezium is present.

New-York academy of sciences.

Jan. 21. — Two specimens of corundum from western North Carolina were exhibited by G. F. Kunz. One was a crystal weighing 13 grams: the other,

weighing $3\frac{1}{8}$ carats, was cut *en cabachon*, and by the exhibitor was said to be the most perfect star-sapphire probably yet found in the United States. Both pieces were a rich light-brown color, very compact, and resembled a variety of sapphire from the hills of precious stones in Siam. Prof. H. L. Fairchild delivered a lecture on methods of animal self-defence, which was well illustrated with a large series of well-selected lantern-slides. Remarks were made by President Newberry and Prof. W. P. Trowbridge.

Canadian institute, Toronto.

Jan. 19. — Prof. R. Ramsay Wright gave an account of researches on the skin and nervous system of *Amiurus catus*, which will shortly be published in the Proceedings. Special attention was devoted to the 'clavate' cells of the epidermis, to the branching of the fifth nerve, and to the relationship between the air-bladder and auditory organ.

Natural science association of Staten Island, New Brighton.

Jan. 12. — Mr. C. W. Leng read a paper on the Cicindelidae of Staten Island. The beetles live all summer in sunny places in the woods, roadsides, and on the sands at the seashore. They are able to make short flights, which they do at the least alarm, flying a few paces at a foot or two from the ground, and then dropping quite suddenly. Their colors always mimic the places at which they are found, which makes it difficult to distinguish them after they alight. During the night and rainy days they hide in holes dug in the sand, or among piles of chips and bark. The following eight species have been found on Staten Island: *C. sexguttata*, *purpurea*, *generosa*, *tranquebarica*, *repanda*, *hirticollis*, *dorsalis*, *punctulata*.

Remarks were made by Mr. Sechusen on a very interesting series of precious stones, their particular characters, and the localities from which they came.

Society of arts, Massachusetts institute of technology.

Jan. 10. — Dr. Charles S. Minot, of the Harvard medical school, read a paper giving an account of his researches on growth and death. Dr. Minot has undertaken an extensive series of experiments, which will occupy many years, on senescence, or the process of growing old; and he presented in this paper the results of one branch of his investigations, in which he had studied the growth of the guinea-pig from birth until the attainment of the full size, having made about fifty-five hundred weighings of these animals, the weight being the function of growth best adapted for study.

In 151 recorded births, the proportion of males to females was found to be 119 to 100. The average weight at birth was about 72.5 grams, and was about the same for males and females. The range was from 42 to 99 for the males, and 46 to 111 for the females. The most potent influence on the weight at birth was found to be the number in a litter; the larger the litter, the less the average weight. The period of gestation was found to average 67 days; and the longer the period, the greater the weight at birth.

This weight was also greater in summer than in winter. Notwithstanding the great variation of weight at birth, the difference diminished with age, all the animals thus tending to approach a certain standard size. One of the most important facts discovered was, that the rate of growth diminishes continuously from the time when the animal recovers from the loss of weight at birth; this diminution being rapid at first, and slower afterward. By rate is here meant, not the absolute increment in weight in a given period, but the per cent of the weight at the beginning of the period, which is added to said initial weight during the period. A discussion of the best available data indicates the result to be also true of man.

Dr. Minot had also made some experiments with rabbits, and compared the results with those for guinea-pigs and for man. He found that the guinea-pig grows on an average, until it is full-grown, 1.73 grams per diem, the rabbit 6.20, and man 6.60 grams. Men are therefore larger than rabbits, not because they grow faster, but because they grow longer; while rabbits are larger than guinea-pigs because they grow faster. The rate of growth, however, as above defined, is very different; being 4.6 % in the guinea-pig, 5 % in the rabbit, and 0.02 % in man.

Phi'sosophical society of Washington.

Jan. 5. — Prof. J. R. Eastman discussed the Rochester (Minn.) tornado of Aug. 21, 1883, describing the ground as it appeared a few days after the storm, and showing that the phenomena did not indicate cyclonic motion. All disturbed objects were thrown in essentially the same direction, and were pressed down rather than lifted. In the course of the ensuing discussion, Mr. W. H. Dall described

similar phenomena in the Escanaba region, where he observed storm-tracks consisting of swathes of prostrate trees, the trunks of which pointed uniformly in one direction.

Mr. Dall then read a paper on Recent advances in our knowledge of the limpets, summarizing the researches of Spengel on the sensory organs or osphradia; Cunningham, on the renal organ and renopericardial pore in Patella and Patina; Fraissé, on the eye in Patina, Fissurella, and Haliotis; and the speaker, on the presence of an intromittent male organ in Cocculina. He stated that among the Acmaeidae and Patellidae the type of eye differs; and while in Patina it is of a very rudimentary character, in other genera it might be well developed, — as, for instance, in Ancistromesus, which has as well developed eyes as Fissurella. He also alluded to the gradual progress in classification afforded by anatomical investigation during the past few years, and observed that nearly all the known forms except Propilidium and Scutellina were amenable to classification; our ignorance of the branchiae in the former, and the dentition in the latter, operating to prevent a final classification in these two cases until more is known. Those authors who study the embryology and histology usually from a single species generally ignore the wide differences of adult anatomy between the genera of limpets, and sow their generalizations on a basis of classification which is little in advance of that of Lamarck and his immediate successors.

The president of the society, Dr. James C. Welling, announced the death, since the last meeting, of Gen. A. A. Humphreys, one of the founders of the society, and pronounced a brief eulogy on his character.

INTELLIGENCE FROM AMERICAN SCIENTIFIC STATIONS.

GOVERNMENT ORGANIZATIONS.

Geological survey.

Geological notes. — Prof. I. C. Chamberlin and his assistants, during December, 1883, were engaged in field-work in Illinois and in Missouri. Professor Chamberlin devoted his personal attention mainly to the borders of the newer drift, the concentric morainic belts that lie within it, and the contiguous old drift without it, in north-eastern Illinois. Mr. R. R. Salisbury continued his previous observations of the residuary clays and loess and drift-borders in eastern and central Missouri.

The revision of the manuscript of a report by Mr. J. S. Curtis on the Eureka mines has been completed by Mr. G. F. Becker and Mr. Curtis, and will soon be ready for the printer.

Since the beginning of Prof. R. D. Irving's study of the metamorphic rocks in 1882, he and his assistants have made five hundred thin rock-sections. Of this number, written descriptions of three hundred have been prepared. They include rocks from the

original Huronian, the Huronian of the Marquette and Menominee regions, the Animikie group of the national boundary, the folded schists of the same region, and the crystalline rocks of the Minnesota and Mississippi valleys.

Assistant John Chaplin at Denver has prepared thin sections of all the eruptive rocks collected in the Rocky-mountain district during the past season.

Paleontology. — Prof. L. F. Ward has completed the work of preparing index slips for a catalogue of fossil plants. He has so arranged all of the fossil plants collected from the Laramie and Fort Union groups, that they are in a convenient form for future detailed investigation.

Chemical division. — The analyses of waters from Walker Lake and Walker River have been completed by Prof. F. W. Clarke.

Mr. J. W. McGee, in his examination of the subterranean forest exposed by an excavation on Connecticut Avenue, Washington, D.C. (referred to in *Science* of Nov. 30, 1883), discovered an earthy blue mineral, which was abundantly distributed throughout the

stratum of clay, at the bottom of which the remains of wood were found. This blue earthy mineral has been identified as vivianite by Professor Clarke.

Mr. Hillebrand, in the laboratory at Denver, has been examining the Leadville porphyries with respect to the proportion of precious metals contained in them. He has prepared a new gravity-solution, the borotungstate of cadmium, designed to replace in part the Thoulet solution, which is in some cases inapplicable in the separation of the mineral constituents of rocks. He has also made various qualitative examinations of several minerals new to the west, — tantalates, columbates, and phosphates of rare earths.

Dr. Mellville and Mr. G. F. Becker, at San Francisco, have been investigating some of the chemical relations of quicksilver.

In the laboratory at New Haven, Dr. William Hallock, at the suggestion of Mr. Arnold Hague, has begun a series of experiments upon the artificial production of geysers. Small models have been made that worked admirably with regular periods. An artificial geyser, with a reservoir twenty-five feet deep, has been constructed, and will soon be in working-order. The study of this model will be of exceeding interest.

Topographical notes. — It was hoped that topographical work could be carried on through the winter in Massachusetts; but owing to the continued bad weather through December, especially in the latter part of the month, the work was greatly delayed; and about the middle of the month it was decided to postpone further field-work in the state until spring.

Mr. Willard D. Johnson, assistant topographer, who has been preparing several small local maps in the Mono basin, California, has completed a map of the Parker-creek moraines, and began one of the Leevining-creek moraines. He has been unable to complete the latter on account of unfavorable weather. The map of the Parker creek moraines includes an area of about seventeen square miles, on a scale of four inches to the mile. The general map of the Mono basin covers some two thousand square miles, and the field sheets of the map are upon an approximate scale of one inch to one and three quarters miles. Several points of the transcontinental triangulation of the coast and geodetic survey are included. They give it scale and position. The vertical relief is derived mainly from angles, and a line of levels connects the work with a determined point on the Carson and Colorado railroad. The map exhibits the outline of the ancient expansion of Mono Lake; the outlines of the ancient ice-stream of the adjacent Sierras with their present remnants. Agricultural and grazing lands are shown, and the areas of timber lines also indicated.

Mr. J. D. Hoffmann, in the division of the Pacific, was busy during December, carrying on the detailed survey of the new Idria quicksilver district in California.

Prof. A. H. Thompson, geographer, was occupied during December in the determination of the latitude and longitude of Fort Wingate, New Mexico, which work has been satisfactorily completed.

PUBLIC AND PRIVATE INSTITUTIONS.

Harvard college observatory.

Funds. — In his report to the president of the university, the director of the observatory states that the annual subscription of five thousand dollars, which has been in force for five years, has expired by limitation, and that an attempt to raise a fund of one hundred thousand is meeting with good success, about half having been already obtained.

Variable stars. — The study of the variable stars has been continued by Mr. Chandler. The bibliography is nearly completed, so far as the first extraction of references is concerned. Notes have been prepared to exhibit the evidence of variability which has been published with regard to about twelve hundred stars. This list excludes many cases in which the evidence is entirely inadequate. A table, giving all the published maxima and minima of each of the variables of long period, is now in process of construction. The preparation of this table has led to the important result that an interval of several years occurs in which no observations appear to have been made of about thirty of these objects. About one hundred and forty stars belong to this class; and, since last April, all of them have been observed by Mr. Chandler with the six-inch Clacey equatorial mounted in the west dome. Charts of the vicinity of these variables have been prepared, and some progress made towards their completion. Similar charts have been made for about seventy telescopic stars suspected of variability, and nearly two hundred observations of these stars have been obtained. The color of the variable stars is also estimated, about three hundred observations of this class having already been made. The circular distributed, asking the aid of amateurs and others, in the observation of stars known or suspected to be variable, has, it is believed, secured much valuable co-operation. Numerous replies have been received, and important results have been obtained, especially by Mr. H. M. Parkhurst of New York, and by the Rev. J. Hagen, S. J., of Prairie du Chien, Wis. The great difficulty encountered by most of the observers was that of identifying with certainty the fainter stars, although this is one of the first things that should be learned by any person desiring to do useful astronomical work.

Astronomical photography. — With the assistance of Mr. W. H. Pickering, an investigation was undertaken in astronomical photography. Two objects were kept in view, — first, the determination of the light and color of the brighter stars; and, secondly, the construction of a photographic map of the whole heavens. After numerous preliminary observations, a method was employed by which a photograph of the brighter stars included in about one-twelfth of the entire heavens could be obtained on a single plate. Maps were also obtained, containing a region of about fifteen degrees square, containing stars as faint as the eighth magnitude. The color exercised a marked influence on the intensity of the photographic images, in some cases producing a difference equivalent to four magnitudes. It is thought that

photography may offer the most delicate test we yet have of the color of a star, — differences too small to be perceptible by the eye, becoming distinctly visible in the photographic images.

NOTES AND NEWS.

PROFESSOR MILNE-EDWARDS writes to the Société de géographie in regard to the scientific work of the expedition on the *Talisman*. After having studied the profiles from the African coast into deep water, the vessel recruited at San Jago, Cape Verde Islands, and later at San Vincente; soundings being carried on during the various movements of the vessel, and proving of great interest, as in some cases they did not accord with those on the charts. Branco Island, which has never been visited by naturalists, was carefully explored. The shores are very rocky, and it became necessary to swim ashore, which the temperature rendered rather agreeable. The island is entirely volcanic, and the rocks of a singular nature. Those near the shore were blocks of lava cemented by a sort of calcareous coquina, containing many shells, into a kind of pudding-stone. Others consisted of sea-sand, drifted by the winds to an altitude sometimes of a thousand feet, and changed into solid layers by calcareous infiltration. Vegetation is very sparse, yet the great lizards peculiar to this island were found to be herbivorous. The Sargasso Sea was then examined, and proved to be of great depth, reaching nearly thirty-three hundred fathoms, and the bottom entirely volcanic, with a rather poor fauna. A collection of lava and scoriae was obtained, some of which appeared to be of quite recent origin. There is probably in the Atlantic an immense band of volcanoes extending parallel with the Andean system, perhaps to Iceland, and of which the culminating peaks form the Cape Verde, Canary, and Azores islands.

More than two hundred deep-sea soundings were made before the return of the expedition *via* San Miguel, Azores. Wonderfully rich collections were made, and specimens of the bottom throughout the whole region traversed. The topography of the ocean-bottom hitherto accepted will be considerably modified by these researches. It was expected that Professor Milne-Edwards would address the society on the general results of the work before a general session about Jan. 21, and exhibit at the same time some of the treasures obtained.

— At the meeting of the Paris academy of sciences, Dec. 10, Dr. Hyades gave a summary report on the geological, botanical, zoological, and anthropological work accomplished by the French mission to Cape Horn. In the southern islands of the Fuegian Archipelago the prevailing rocks were found to be schists and granites, greatly weathered wherever unprotected by vegetation. The dwarf Antarctic beech is limited to an altitude of four hundred metres, the *Fagus betuloides* to three hundred, forming with the *Drimys* and *Berberis* a forest zone with a humid soil poor in vegetable humus, and covered with mosses, heaths,

and a considerable variety of small plants. The marine flora abounds in all kinds of algae (the most common being the *Macrocystis pyrifera*), affording a shelter to numerous zoophytes, annelids, mollusks, crustaceans, and migratory fishes of eight or ten species. Of the shell-fish, which abound on most of the seaboard, all the large species are edible. Although poorer than the marine, the land fauna includes several species of Coleoptera, Lepidoptera, Arachnida, some forty species of birds, but no reptiles or frogs. The mammals are represented by only one species of fox, two rodents, and an otter, besides the domestic dog. The natives all belong to the Tekeenika stock of Fitzroy, called Yahgans by the present English missionaries. They speak an agglutinating language, current from the middle of Beagle passage to the southernmost islands about Cape Horn. About one thousand words of this language were collected, including some abstract terms, such as *tree*, *flower*, *fish*, *shell*. The numerals get no farther than three, although the natives count also on the fingers. Over a hundred anthropometric observations were taken on individuals of all ages and both sexes. Good photographs were also obtained of a large number of Fuegians, besides numerous castings of all parts of the body, some skeletons, and a great variety of ethnological materials.

— Besides the analyses of snow made at Madrid and in Holland (in which was observed volcanic sediment similar to that of the ashes found in Java after the eruption of the volcano), mentioned by Mr. Upton in his article on the 'Red skies,' in *Science* of Jan. 11, *Nature* of Dec. 20–Jan. 3 contains a number of letters in which mention is made of a grayish volcanic (?) sediment having been found at several points in England after rain-storms in December.

— The International congress of geologists will meet at Berlin on the 25th of September next, and last five days; then a grand geological excursion will be made through the Hartz Mountains, Saxony Switzerland, from the 1st of October to the 5th, ending at Dresden by a visit to the Royal museum, under the guidance of its celebrated director, Prof. Dr. H. B. Geinitz.

— In Oregon City there is a large apple-tree in the Methodist-church lot, planted in 1842 by W. S. Moss, Esq., for Rev. G. Hines, who was then living there. The tree bears two kinds of fruit, but only one kind each year, and the different kinds appear on alternate years. It is still a vigorous, healthy tree.

— It is understood that the outer satellite of Mars, Deimos, has been observed by Professor Hall during the present opposition. As the planet Mars is now near its aphelion, its visibility would seem to show that the satellite can be observed at every opposition of Mars with the great telescopes which have recently been constructed.

— The Pi eta scientific society of Troy, N.Y., has changed its name to Rensselaer society of engineers.